The Faraday Lecture for 1875.

THE LIFE-WORK

OF

LIEBIG

IN EXPERIMENTAL AND PHILOSOPHIC CHEMISTRY; WITH ALLUSIONS TO HIS INFLUENCE ON THE DEVELOPMENT OF THE COLLATERAL SCIENCES AND OF THE USEFUL ARTS.

A DISCOURSE

Delivered to the Fellows of the Chemical Society of London in the Theatre of the Royal Institution of Great Britain, on March the 18th, 1875.

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Ὄλβιος ὅστις τῆς ἱστορίας
ἔσχε μάθησιν—
ἀθανάτου καθορῶν φύσεως
κόσμον ἀγήρω πῃ τε συνέστη
καὶ ὅπῃ καὶ ὅπως.
τοῖς δὲ τοιούτοις οὐδέποτ' αἰσχρῶν
ἔργων μελέτημα προσίζει.
Euripides.

London:
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1876.

THE FARADAY LECTURE.

We are assembled this evening, friends and fellow-workers, to render our third triennial tribute of homage and gratitude to the memory of Michael Faraday—to keep his great name and example bright among us, and, by so doing, to renew in our minds and hearts the inspiration of his incomparable genius.

In organising this periodical celebration of Faraday's life and labours, the Council of the Chemical Society, mindful of what would have been the wish of the great philosopher himself, has resolved to make these meetings the occasion, not of vain lamentation over an irreparable loss, nor of reiterated eulogies of the great luminary whose setting we deplore, but rather of useful surveys of the fields of science he so dearly loved, and of the lives and labours of those illustrious fellow-workers in whose ranks he so conspicuously shone.

As Faraday belonged, from the universality of the benefits conferred by his genius on the human race, not merely to the island of his birth, but to all the civilised countries of the globe, the Council wisely and generously ordained that all nations should be invited to share with England the happy privilege of rendering homage to the greatest experimental thinker who has ever yet appeared among mankind.

France, worthily represented by the illustrious Dumas, inaugurated, six years ago, the series of these commemorations. In a discourse not less remarkable for the philosophic grasp and grandeur of its conception, than for the harmonious beauty of its flowing periods, he set before us the scientific needs and tendencies of Faraday's time, and the noble labours which placed him foremost among the sowers and reapers of its grand scientific harvests.

Italy followed in the person of Professor Cannizzaro, who discoursed to us, in language alike profound and eloquent, of the form to be impressed upon the future teaching of chemical science, thus developing a theme, not only in itself of the deepest interest, but one peculiarly appropriate to the occasion; since Faraday, even had he not as a discoverer won imperishable renown, would have ever stood high among the promoters of scientific knowledge by his inapproachable power as an expositor of philosophical truth.

On the present occasion Germany has been invited to take her part in this international tribute to departed genius; and I shall ever account it one of the most signal honours of my life, that I have been selected to appear before you here, the spokesman of my country's chemists—the interpreter of

German reverence for the greatest physical philosopher of the age.

I need hardly dwell on my deep sense of the magnitude of the duty I have undertaken to discharge, and of my inadequacy to the task of worthily continuing the series of discourses so admirably commenced by my distinguished predecessors.

Conscious of my inferiority to them in the graces of style and the charm of oratory, I have anxiously selected as the theme of my discourse a subject so intrinsically rich in interesting facts and noble lessons, as of itself to command and repay your attention, while claiming from me no eloquence beyond that of a succinct and faithful exposition.

Such, Gentlemen, are some of the considerations which have determined me to direct your attention to the labours of one of Faraday's most eminent scientific contemporaries—of a master-mind gifted as Faraday's own,—of my illustrious teacher and deeply lamented friend—JUSTUS VON LIEBIG.

In addressing myself to this task,—in proceeding to lay before you a sketch of the labours of Liebig, and in touching on some of the characteristic incidents of his career,—I find myself embarrassed by the very richness of the subject it is my duty to unfold.

The many-sided genius of the great discoverer baffles, with its prolific outpourings, my sense of order and selection; so that I know not how to discriminate, amidst the manifold treasures he has bequeathed to us, those which claim development on this occasion, from those which, through sheer lack of time, must pass unnoticed.

Let me, however, at starting, frankly declare to you my deep-rooted conviction that Liebig's is the name and figure alone fitted to stand beside Faraday's in the representation of our century to future generations of mankind. Indeed, even while I say this, I am but too well aware that it is hardly for us, their contemporaries, to comprehend, in all its fulness, the towering majesty of these two great men.

As those who wander in a mountain-chain cannot appreciate the sky-reaching grandeur of its lofty peaks so well as those who, remotely, from the plains beneath, contemplate its snow-crowned summits, so we, the contemporaries of Faraday and Liebig, cannot perceive the full dignity of their commanding forms,—the philosophic pinnacles of this century,—as they will hereafter appear to distant generations of posterity. In those days Faraday and Liebig will be looked up to with such reverence as it is ours to offer to the mighty spirits of the past—to such giant figures as those of Galileo, Kepler, Newton, and Lavoisier. And as that bright constellation shines on us from the misty darkness of the past, so will the names of Faraday and Liebig,—stars of co-equal lustre,—throw forward their bright beams on our successors through the far-reaching vista of ages yet to come.

To speak of Liebig only: were we to consider merely the vast number and incalculable importance of the chemical facts which he established, we should have to proclaim him one of the greatest contributors to chemistry at large, that ever has appeared; while of organic chemistry we could not hesitate to consider him the very source and fountain-head.

Yet the discovery of chemical facts has been but a part, and not the greatest, of Liebig's memorable services to our cherished science. By his experimental studies on the correlation and mutual bearing of the facts he discovered, he was led to the conception of general laws, which have shed a flood of light on chemical phenomena of all classes; illustrating no less the course of inorganic transformations, than the nature of organic compounds and their activities—the field he especially cultivated. By the great types of composition which, under the name of radicles, he was the first to reveal, and by the general methods of research resulting from their recognition, he was enabled, not only to trace with a sure and cautious hand the lines of his own lifelong progress, but to map the path of all contemporary research, and shape the course along which, from age to age, so long as chemistry and the collateral sciences continue to advance, they must pursue their development.

Speaking, as I do, in the presence of chemical investigators to whom Liebig's methods and apparatus are so familiar, I need spend no time on the proof of what I have just advanced. Such proof we find, each one of us, day by day, at every step of our experimental researches. Nor is this hourly help, great as we feel it to be, and masterly as is the hand to which we owe it, by any means all the aid we chemists daily derive from Liebig. It was he who, while placing at our disposal the means, intellectual and material, of prosecuting our researches, was also the first to found in Europe the great institutions for chemical education, by which our minds have been prepared and equipped to employ with advantage the keen weapons, theoretic and instrumental, provided by this great exemplar for our use.

It was at the small University of Giessen that Liebig organised the first educational laboratory, properly so called, that was ever founded. The foundation of this school forms an epoch in the history of chemical science. It was here that experimental instruction, such as now prevails in our laboratories, received its earliest form and fashion; and if, at the present moment, we are proud of the magnificent temples raised to experimental science in all our Schools and Universities, let it never be forgotten that they all owe their origin to the prototype set up by Liebig half a century ago. The new school called around the master, from all nations, a large number of pupils, the *élite* of the then rising generation of chemists, many of whom are now, in their turn, distinguished masters of our science, having worthily continued the path of discovery opened for them, in their youth, by Liebig. It was more especially from this country that a great number of young chemists thronged to the school of Giessen; and I see many before me who not only enjoyed its educational advantages, but have since nobly illustrated their value by the high eminence which they have attained; and I well know that if their gratitude towards Liebig had to be expressed by acclamation, there is not one of those old pupils present whose voice would be wanting in the general tribute of heartfelt reverence and praise to the great master.

This is a point on which, brief as our time may be, I cannot, in common duty, lightly dwell. It is, in fact, one of the most characteristic features of Liebig's influence on the development of modern philosophy, as well as one of the grandest and most generous endowments of his princely heart.

Whoever has had the good fortune of attending his lectures, will not easily forget the deep impression of his peculiar style of eloquence. Liebig was not exactly what is called a fluent speaker; but there was an earnestness, an enthusiasm in all he said, which irresistibly carried away the hearer. Nor was it so much the actual knowledge he imparted which produced this effect, as the wonderful manner in which he called forth the reflective powers of even the least gifted of his pupils. And what a boon was it, after having been stifled by an oppressive load of facts, to drink the pure breath of science such as it flowed from Liebig's lips,—what a delight, after having perhaps received from others a sack full of dry leaves, suddenly, in Liebig's lectures, to see the living growing tree!

Yet not, however, in lecturing was it that Liebig most excelled; it was by the peripatetic teaching in the laboratory that his greatest successes were achieved.

Like all the great generals of every age, Liebig was the spirit as well as the leader of his battalions; and if he was followed so heartily it was because, much as he was admired, he was loved still more. If I speak somewhat fondly of Liebig, many around me are thinking of him fondly too, for we were alike pupils of his. We remember his fascinating control over every faculty, every sentiment that we possessed; and we still, in our manhood now, remember how ready we were, as Liebig's young companions in arms, to make any attack at his bidding, and follow wherever he led. We felt then, we feel still, and never while we live shall we forget Liebig's marvellous influence over us; and if anything could be more astonishing than the amount of work he did with his own hands, it was probably the mountain of chemical toil which he got us to go through. I am sure that he loved us in return. Each word of his carried instruction, every intonation of his voice bespoke regard; his approval was a mark of honour, and of whatever else we might be proud, our greatest pride of all was in having him for our master. It was our delight, too, to know that we helped him; that while we received his lessons, we were also performing his work. The aid he thus obtained, he was too just ever to deny or underrate; nay, his generosity often attributed to a pupil the whole credit of a successful experiment suggested by himself, on the basis of previous trials and discoveries of his own, and of his deductions therefrom. Of our young winnings in the noble playground of philosophical honour, more than half were free gifts to us from Liebig; and to his generous nature no triumphs of his own brought more sincere delight than that which he took in seeing his pupils' success, and in assisting, while he watched their upward struggle.

Not only then has Liebig, by his multitudinous discoveries of facts, laid the foundation of organic chemistry; not only, has he, by his conception of chemical radicles, and his keen insight into chemical analogies, marked out in theory the way of chemical research and discovery for centuries yet to come; not only has he, in addition to his theoretic guidance, given us the instruments and means of prosecuting the researches by which the domain of chemistry must be enlarged; but he has also shown us how to keep up the supply of intellectual agents to carry on the work, how human hearts and minds may be prepared to prosecute the great warfare against ignorance in this field of science,—thus furnishing trained soldiers to wield the arms he previously provided.

I am sure, Gentlemen, that I shall have your unanimous concurrence when I say that there is no greater proof of the fecundity of a discoverer's genius than this,—that it not only itself raises the curtain from Nature's secrets, and enriches the storehouse of science from the vast treasury of the previously unknown, but that it also endows mankind with the means, intellectual and material, of following on in the same path; thus taking an anticipated part in Time's ulterior conquests.

And to all these great services done to our race by Liebig, may I not truly add the inspiration bequeathed to us by his illustrious example? Which of us, returning to-morrow to his lonely post in the laboratory, and resuming his obstinate labour in penetrating Nature's stubborn depths, will not feel animated and cheered in his work by the example of such master-minds as those of Faraday and Liebig? For us chemists more especially, and for our successors, it will be for years to come a duty, equally imperative and delightful, to work, not only with Liebig's instruments in our hands, but also with his noble spirit in our hearts.

And that spirit, as characteristic of Liebig's genius as of Faraday's, led both those great men, as I trust it may lead us, their humble followers, to look beyond the scope of a single sphere of thought; to pass from the discovery of special instances to the determination of laws governing whole classes of a like nature, and to trace forward those laws in their influence on still wider ranges of phenomena; not neglecting at the same time their collateral applications in the improvement of the arts of life, and in the promotion of man's material welfare.

Were Faraday the theme of my discourse, I could truly say that no man, more abundantly than he, has enriched, incidentally, the collateral fields of industry by the activity of his pursuit after the abstract laws of nature. And it is but one more of the noble analogies traceable between the careers of these two great men, that Liebig's labours in abstract science have also, like Faraday's, borne copious fruit in many of the useful arts, especially in those whose practice involves chemical transformations.

To mention but a few of Liebig's services of this kind, let me remind you of the great industries of acetic acid and the fatty bodies, which were materially elucidated, as well by Liebig's own researches, as by those performed in his laboratory by pupils under his immediate guidance. Among the manufactures thus in some cases brought to a degree of perfection previously unknown, in other cases actually created by him, you will all recollect, as prominent examples, the industry of the fulminating compounds, that of prussiate of potash, and lastly that of potassic cyanide. Of these, the two former owe to Liebig's labours the very key to their operations; whilst the latter originated entirely from his investigations in the cyanogen group. Indeed potassic cyanide, but a few years ago a substance of exclusively scientific interest, is now, since Liebig devised an easy mode for its preparation, a commercial article of considerable importance, large quantities of which are used in the various processes of electroplating. As not less intimately, though somewhat differently, rooted in Liebig's researches, may be mentioned the invention of silver-coated mirrors, so superior in effect to the old mercurial reflectors, and now, as we all know, manufactured so extensively.

I might greatly prolong this enumeration; but enough, I think, has been adduced to justify me in stating that Liebig, like Faraday, merited the old classical encomium, *illustrans commoda vitæ;* and never, in his conscientious benevolence, lost sight of the general interests of his race.

It was not, however, incidentally only, by the collateral development of the industrial arts, that the practically beneficent outcome of Liebig's genius was displayed. In fathoming the deep mysteries of organic chemistry, his penetrating curiosity could not remain indifferent to the yet more profound secrets of biology, that is to say, of life, in its two great forms, vegetal and animal, based, as they both are, in their material development, on processes of chemical change.

In the laws of plant-life more especially, Liebig's noble researches threw ray after ray of brilliant light into depths where before the deepest obscurity had reigned. It was Liebig who traced the primordial conditions of the nutrition and growth of plants; and finally established their connection with the chemical composition of the soil in which they are rooted, and of the air in which their leaves are bathed, as well as with the imponderable forces, especially the sun's light and heat, under whose influence they live.

Projecting his view still further in the same direction, he also traced the influence of physical and chemical laws on the second and higher division of biology, namely, that which relates to animal life, its laws and conditions, especially those of the nutrition and development of the animal body.

It was no doubt in the former of these two high and arduous paths of research that Liebig's labours were crowned with the most perfect success. Undertaken in the year 1837, at the request then made to him by the British Association for the Advancement of Science, for a Report on the state of our knowledge in organic chemistry, they led him to the publication in 1840 of his memorable work, entitled "Chemistry in its Applications to Agriculture and Physiology."* Twenty-two years later (1862), having, during this long interval, studied in minute detail the several questions connected with the subject, he issued his invaluable work, "The Natural

* *Die Chemie in ihrer Anwendung auf Agricultur und Physiologie, von Justus Liebig, Professor der Chemie an der Universität Giessen. Braunschweig*, 1840. Chemistry in its Applications to Agriculture and Physiology. By JUSTUS LIEBIG, M.D., Ph.D., F.R.S., M.R.I.A., Professor of Chemistry in the University of Giessen. Edited from the manuscript of the Author, by LYON PLAYFAIR, Ph.D. London, 1840.

Laws of Husbandry;"* and his researches, as embodied in this great work, may truly be described as constituting the first perfect construction of the philosophy of agriculture that had ever appeared up to that date. Had this memorable treatise been Liebig's only work, it would have secured to him an imperishable fame, associating him, as it did, with his most illustrious predecessors in chemico-biological inquiry—I mean, as you all well know, Lavoisier and Humphry Davy.

Two years had scarcely elapsed since the publication of his first treatise on agricultural chemistry when Liebig issued his memorable work, "Organic Chemistry in its Applications to Physiology and Pathology,"† in which were embodied his first results in the second and superior branch of chemico-biological research,—that branch, namely, which brings animal vitality within the scope of nature's general laws; and from that period (1842) to the time of his

* *Die Chemie in ihrer Anwendung auf Agricultur und Physiologie, von Justus Liebig. 1. Theil. Der chemische Process der Ernährung der Vegetabilien. 2. Theil. Naturgesetze des Feldbaus. Braunschweig*, 1862. The English edition of Part II. is entitled, "The Natural Laws of Husbandry," by JUSTUS VON LIEBIG. Edited by JOHN BLYTH, M.D., Professor of Chemistry in Queen's College, Cork. London, 1863.

† *Die Thierchemie, oder die Organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, von Justus Liebig. Braunschweig*, 1842. "Animal Chemistry, or Chemistry in its Applications to Physiology and Pathology." By JUSTUS LIEBIG. Edited from the Author's manuscript, by WILLIAM GREGORY M.D., F.R.S.E. London, 1842,

death (in 1873), this noble theme never ceased to occupy his thoughts.

In these three splendid works, each, if I may use the expression, a conqueror's battle-field, Liebig built up new kingdoms on the ruins of empires overthrown. The vague hypotheses of old days fell, like captured forts, before him; and, grimly potent as their defences might have seemed, he showed them to be founded, not on solid facts, but on fallacious guesses only. They, one by one, broke down under the severe philosophical analysis to which they were submitted by Liebig, and under the crucial testing by actual experiment which they underwent at his hands. In the study of biology, vegetal and animal, Liebig was the first to disentangle intricacies that had before seemed problems beyond the grasp of human intellect to solve; and it was one of the grandest results of his philosophical and experimental investigations, that he traced, amidst the multitudinous and apparently ever-varying manifestations of life, in its countless modifications of kind and degree, the operation of a few simple laws, physical and chemical, affording, by their determinate combination, the precise and proved conditions of vital development, nutrition, growth, and perpetuation, from generation to generation, in unaltered individuality.

In the vegetal division of these biological researches, not one of more profound importance can be referred to, among Liebig's grand achievements, than his clear and well-established recognition of the necessity, for plant-growth, of the ingestion of the minute percentage of non-volatile saline ingredients which remain as the "ash" of every plant when burnt. To Liebig we owe the now irrefragably established knowledge that each one of these saline ingredients, however minute its percentage-proportion in the composition of the vegetal tissue, is as essential to the plant's life and development as is a full supply of the most weighty constituents of its organic mass. And as a corollary of this universal law, he deduced the certain knowledge that these saline ingredients, taken from the soil by food-crops, unfit the land for the further growth of such crops until those saline ingredients, or ash-constituents of the plant's organised tissue have been restored to the field; a well established law, whence has sprung the most important rule of agricultural economy, namely, that to maintain the fertility of the crop-producing fields, so much ash-ingredient must be annually restored to the ground as the crop, taken from it, has withdrawn. Pressing his investigations further in this direction, Liebig urged eloquently, not upon philosophers only, but also on practical economists, on statesmen, and on the agricultural community at large, the incalculable waste of national resources, arising from our ignorantly treating town-sewage as a worthless refuse, though it is charged with the saline residue of the citizen's food,—wastefully diverting it from the land and pouring it down the rivers to the sea.

The Malthusian economists, observing as a fact the growing scarcity of food, but equally ignorant of the cause of the evil, and of the means available for its cure, did not hesitate to predict the unavoidable arrest, at no distant date, of our race's growth and development, "by the ever-increasing pressure of expanding population on the limited means of subsistence." But science, meantime, speaking with Liebig's voice, overruled these gloomy forebodings, and showed the collective organism of mankind to be a self-supporting institution. On this momentous subject, with the very existence of humanity at stake, Liebig did not always speak calmly, but he always spoke wisely and well.

In language of impassioned eloquence, he protested against the universal impoverishment of the food-producing continents of the globe by this perpetual robbery and waste of the essential conditions of fertility; and he warned the wealthy population of Great Britain, in particular, that their present reckless annual waste of the national resources was hurrying them along the downward road to general decay and impoverishment, which not all the British treasures of coal, iron, and other grand elements of power and property would ever suffice to redeem. As a provisional means of postponing, though not of preventing, the arrival of these foreseen calamities, Liebig was led to make one of his most remarkable suggestions of improvements in practical industry, namely, that of manufacturing artificial fertilizing compounds, rich in the saline or ash-ingredients of plants, and therefore adapted to renew provisionally the fertility of soils exhausted by the annual exportation of their ash-containing crops. It is true that the proposal of Liebig's, vast as is the industry to which it has given rise, cannot be of enduring value to mankind, since the artificial restitution of the saline ingredients to the soil must be attended by an annual waste equal to that occasioning the original impoverishment of the soil, and certain, at no distant date, to exhaust the chemical resources at disposal for artificially repairing that waste.

Be this, however, as it may, the whole history of philosophical research, and of its influence in the guidance of industry, does not furnish any example of nobler and grander results than those which have ensued, and are still to follow, for the benefit of mankind, from the splendid generalisation of Liebig's pregnant thought upon this subject. It has supplied to agriculture the fundamental art of life, its main basis as a perdurable art, as an industry no longer liable to extinction by the exhaustion of the soil; and it has enabled us—let me repeat it—to secure not merely the continuous regeneration of plants, but also the ceaseless perpetuation of the animal race, including its chieftain, man—a chain of incommensurable importance, whose first link hangs, if I may so speak, from Justus Liebig's hand.

In the second division of biology,—in that great branch of science which leads us from the study of the lower life of plants to the higher vitality of animals and man,—Liebig's labours effected a revolution not less complete and momentous than that which ensued from his researches in agricultural chemistry. The student of the laws of animal life and of the changes occasioned in normal vital operations by the influence of disease, was, before Liebig's time, but little accustomed to apply the accurate methods of chemical and physical investigation to the complicated problems involved in vital processes; and when, occasionally, chemical comments were introduced into physiological discussions, they were usually of the most vague and hypothetical description.

This disregard of the chemical method was due mainly to two causes: in the first place, to the circumstance that this method had not at that time arrived at the degree of elaboration and perfection which it has since attained; and secondly, and perhaps chiefly, to the reluctance felt by the inquirers of that period to divest any processes, however simple, when accomplished within the living organism, of the operation of vital force. This force of vitality was a kind of bugbear, deterring the majority of investigators from engaging in the study of animal chemistry; since it was generally believed that all manifestations of chemical and physical action were modified and overruled in a most unaccountable manner by this mysterious force. It is true that many steps towards a salutary change in this view of things had been made long before Liebig took the field. Several of the constituents of the animal economy had been examined with more or less success. The physiologists here present will think of Chevreul's classical labours on the fatty bodies of animal origin; of the early investigations by Berzelius of urine, blood, and bile; and of Gmelin and Tiedemann's researches as to the nature of digestion. Again, some of the crystalline compounds occurring in animal fluids had been correctly analysed. Thus William Prout, as early as 1819, had fixed the formula of urea which is in use at present; and nine years later, in 1828, Wöhler had demonstrated the possibility of building up from its elements this very urea, the formation of which, up to that period, had been supposed to take place exclusively under the influence of vitality,—an experiment ever memorable, since it removed at a single blow the artificial barrier which had been raised between organic and inorganic chemistry. And to mention another somewhat earlier observation of the same discoverer, which, more perhaps than any other, contributed to shape the course of Liebig's researches: Wöhler in 1824 had proved that the salts of organic acids, by passing through the animal body, are converted into carbonates, undergoing, in fact, the same change which is effected by their combustion in the open air. It would not be difficult to quote additional interesting investigations of animal products and processes but the results obtained had remained unconnected. No one had ever ventured to collect these scattered efforts into a focus for the general elucidation of the phenomena of animal life. It was reserved for Liebig to accomplish this arduous task. Amidst the complex and apparently entangled phenomena attending the development and maintenance of animal vitality, Liebig was the first to discern and elucidate the precise and determinate action of chemical and physical laws. This great conception, which, before his day, had never illumined physiological science, germinated in Liebig's mind, was proved and expanded by his experimental investigations, and, finally, by his energetic teaching, became established as one of the convictions of our race.

It was under auspicious circumstances that the great task was attempted. Liebig was then in the zenith of his intellecual career; and if his special object had been to prepare himself for this order of inquiry, he could not have selected more appropriate work than that which during the twenty years previously he had been in the habit of performing. The method of analysing organic bodies was at that time already elaborated; nor had any one chemist then living more experience in its use; and no one, lastly, was surrounded by a greater number of pupils desirous and well qualified to aid him in his researches. Moreover, it was certainly of no small importance, that, at the period in question, Liebig had already achieved some of his splendid results in the collateral branch of agricultural chemistry; and if his preliminary studies in the animal physiology of the time had not been carried to the extreme of detail, we may ask whether this was not an additional advantage, since it enabled him to proceed, unburthened with observations in many cases doubtful and perplexing, and untrammelled by the fetters of preconceived notions.

But let us examine some of Liebig's chemical work in animal physiology.

One of the earliest subjects to which Liebig devoted himself in this department was the question as to the origin of animal heat. After Lavoisier had recognised the analogy between the processes of combustion and respiration, the idea naturally suggested itself to his mind that the heat observed in the animal organism must have the same origin as that which is evolved during combustion; and afterwards, in a paper published jointly with Laplace, these two illustrious philosophers distinctly stated it as their opinion, based upon experiment, that the sensible heat of the animal organism is the combustion-heat of the carbon and hydrogen which, in the form of food, are burnt in the body. The view advanced by Lavoisier and Laplace was subsequently tested by Dulong and Despretz in a series of admirably conducted researches, performed with all the resources the advancement of science had meanwhil placed at their disposal. These physicists proved that a very considerable proportion indeed of the heat observed may be thus explained, but that, at the same time, an appreciable amount (from 10 to 11 p.c.) remains unaccounted for; and it marks the state of inquiry at this period that, for the purpose of explaining this deficiency, physiologists did not hesitate for a moment to invent additional sources of animal heat, such as nervous activity, friction, and electrical phenomena taking place within the living organism. By a careful revision of the subject, and by introducing into the calculation of the experiments the combustion-heats of carbon and hydrogen as furnished by the latest estimations, Liebig arrived at the determinate conclusion that the deficit which even yet remained is to be attributed to a diminution of temperature, which the animal must have suffered whilst in the ice-calorimeter,—an assumption which appears the more legitimate since the belief prevailing at one time that the temperature of the animal body is a constant one, might easily have given rise to what now would appear a strange omission in the experiments. Be this, however, as it may, in summing up the inquiry, Liebig declares it to be his unalterable conviction that the whole of the sensible heat of the animal body may be explained by processes of combustion accomplished within the organism. The assumption of sources of animal heat other than chemical now belongs to the past.

It is true, that with the mechanical theory of heat, such as it has been developed by modern physics, for the foundation of all our considerations, the question at issue assumes a much less important aspect; for even admitting, as we may do, that nervous activity, friction, and electrical phenomena actually give rise to evolution of heat in the animal body, we now know that all these actions are themselves but intermediate expressions of chemical change resulting eventually in heat. It is certainly a welcome proof of progress that truths, which the inquirers of a previous period had to conquer step by step, have become, as it were, self-evident to us. But if to-day we rejoice to look down from a greater altitude, we certainly shall not cease gratefully to remember those whose labours have contributed to lift us to our higher point of view.

In thus studying the evolution of heat by the oxidation or slow combustion of food in the animal organism, a mind like Liebig's could not but be drawn to investigate the nature of food itself, and his high generalising power soon led him to his well-known classification of nutritive substances. Having duly dwelt upon the importance of the mineral constituents of food,—the so-called nourishing salts which previously had been entirely disregarded, and the co-operation of which, in the building up the animal body, is not less essential than in the development of the plant,—he proceeds to classify food according to the special purposes which it is to fulfil in the animal economy. "The food of man and animals," he says,* "consists of two classes of substances essentially different in their composition. The one class (consisting of nitrogenous substances, albumin, etc.), serves in the formation of blood and in building up the various organs of the body; it is called plastic food. The other (consisting of non-nitrogenous substances, the fatty bodies, and the so-called carbo-hydrates), resembles ordinary fuel, serving, as it does, in the generation of heat; it is designated by the term respiratory food. Sugar, starch, and gum may be looked upon as modified woody fibre, from which, it is known, they are capable of being formed. Fat, by the quantity of carbon it contains, stands nearest to coal. We heat our body, exactly as we heat a stove, with fuel which, containing the same elements as wood and coal, differs essentially, however, from the latter substances, by being soluble in the juices of the body."

It is not often that new conceptions, so utterly at variance with what had been previously believed, can boast of such rapid and general reception as was accorded to Liebig's classification of nutriment into nitrogenous, or plastic, and non-nitrogenous, or respiratory.

Of course this, like all other classifications ever

* *Ueber die Verwandlung der Kräfte. Sammlung wissenschaftlicher Vorträge gehalten zu München im Winter*, 1858. *Braunschweig*, p. 594.

proposed, is not a perfect one; for it would be difficult to draw an accurate line of demarcation between the two classes. But these imperfections cannot possibly diminish the value of a view which more perhaps, than any other, has contributed to a correct appreciation of the process of animal nutrition. Some objections have been recently raised against Liebig's classification, more especially on physiological grounds, and it is, therefore, of particular interest to listen to the opinion which one of the most illustrious physiologists of the day, Theodor von Bischoff, has but lately expressed upon this subject.*

"These objections," he says, "undoubtedly correct as they are, have not been able to supplant Liebig's views, nor will they ever be able to do so, for the truth of these views as a whole will always remain; nor is it possible to deny the great merit they possess of pointing out in the briefest manner the essential differences of the several varieties of food. In the division, classification, and designation of natural objects, and even of historical events, supposing them founded, not on nature but on the requirement of a comprehensive discriminative distinction, the principle has ever been acknowledged: *a potiori fit denominatio;* and it is this principle which justifies Liebig's classification and confers on it its value."

To the active prosecution of the study of food

* *Theodor L. W. Bischoff, über den Einfluss des Freiherrn Justus von Liebig auf die Entwickelung der Physiologie. München*, 1874, p. 27.

from the chemical point of view, we are indebted for the rapid development of one of Liebig's finest philosophical conceptions, namely, his theory of the nutrition of animals. According to this theory, the plant holds a position intermediate between the mineral and the animal world. The animal is incapable of assimilating the compounds stored up in inorganic nature. To render these compounds subservient to the purposes of animal life, they have to undergo a preliminary preparation within the living organism of the plant. The simple mineral molecules are thus converted into molecules of a higher order, fit to serve in building up and maintaining alive the body of the animal. The main pillar of this theory is the fact established by experiment, that the nitrogenous principles composing the body of the animal, animal albumin, animal fibrin, and animal casein, are identical in composition with the nitrogenous principles found in the organism of the plant, vegetal albumin, vegetal fibrin, and vegetal casein. The similarity of some of these substances, of animal and vegetal albumin, for instance, had even previously been pointed out by Mulder; but certainly it was left to Liebig to prove their identity by analyses performed either by himself or by his pupils. From the body of the animal, the mineral matter organised by the intervention of the plant, after having served the purposes of animal life, returns again to the stores of mineral nature, in order to renew the circulation.

I cannot deny myself the pleasure of reminding you of the often-quoted passage in his Familiar Letters on Chemistry,* in which Liebig eloquently sets forth his views :—"How admirably simple," he says, "after we have acquired a knowledge of this relation between plants and animals, appears to us the process of formation of the animal body, the origin of its blood and of its organs! The vegetable substances, which serve for the production of blood, contain already the chief constituent of blood ready formed, with all its elements. The nutritive power of vegetable food is directly proportional to the amount of these sanguigenous compounds in it; and in consuming such food, the herbivorous animal receives the very same substances which, in flesh, support the life of carnivora.

"From carbonic acid, water, and ammonia, that is, from the constituents of the atmosphere, with the addition of sulphur and of certain constituents of the crust of the earth, plants produce the blood of animals: for the carnivora consume, in the blood and flesh of the herbivora, strictly speaking, only the vegetable substances on which the latter have fed. These nitrogenised and sulphurised vegetable products, the albuminous or sanguigenous bodies, assume in the stomach of the herbivora the same form and properties as the fibrin of flesh and animal albumin do in the stomach of the carnivora.

* Familiar Letters on Chemistry, 3 edit., 1851, Letter xxvi, p. 350.

"Animal food contains the nutritive constituents of plants, stored up in a concentrated form.

"A comprehensive natural law connects the development of the organs of an animal, their growth and increase in bulk, with the reception of certain substances essentially identical with the chief constituent of its blood. It is obvious that the animal organism produces its blood only in regard to the form of that fluid, and that nature has denied to it the power of creating blood out of any other substances, save such as are identical, in all essential points, with albumin, the chief constituent of blood.

"The animal body is a higher organism, the development of which begins with those substances, with the production of which the life of those vegetables which are commonly used for food ends. The various kinds of grain and of plants used for fodder, die as soon as they have produced seeds. Even in perennial plants, a period of existence terminates with the production of their fruit. In the infinite series of organic products which begins with the inorganic food of plants, and extends to the most complex constituents of the nervous system and brain of animals the highest in the scale, we see no blank, no interruption. The nutritive part of the food of animals, that from which the chief material of their blood is formed, is the last product of the productive energy of vegetables."

It is impossible to speak of Liebig's achievements in physiological chemistry without alluding to his doctrine of the origin and function of fat in the animal economy—questions which had never been duly considered at the time when he engaged in this line of inquiry. A careful investigation of the conditions under which accumulation of fat is observed in the body, led him to the positive conclusion that it is within the animal organism that the elaboration of fat takes place; and that the materials consumed in its formation are the carbo-hydrates, such as starch and sugar, &c., which, like the nitrogenous principles also, the animal finds ready prepared and stored for its use within the organism of the plant.

The views advanced by Liebig gave rise to a long and animated controversy with some of the leading chemists of France, more especially with Dumas and Boussingault, who contended that the animal received the fat ready formed from the plant. An appeal to experiment, it is well known, has decided the question in favour of Liebig, and it is interesting to note that in this case, as in many others, the most powerful arguments in support of his views were brought forward by his antagonists themselves. "My mill has ever received its best supply of water from my opponents," Liebig used to say. Their experiments proved indeed that the vegetal food of animals contains more fat than had previously been believed; but an amount nevertheless utterly insufficient to explain the quantity deposited in fattened pigs and geese living entirely on vegetal food. Important collateral proof of the faculty possessed by the animal economy of transforming sugar into fat was supplied, moreover, by the observation that bees, when fed exclusively on sugar, nevertheless continue to produce their wax, the homology of which with the ordinary fatty substances had been established beyond doubt by Brodie's researches; and, as if to complete the chain of evidence, Pelouze, in the very nick of time, had demonstrated the transformation, by the intervention of cheese-ferment, of sugar into butyric acid, thus experimentally proving the process assumed by Liebig to be accomplished within the organism of the animal. I am not permitted here to develop this subject fully in its various ramifications; else I should have also to point out the modifications which Liebig's views, as originally proposed, have since undergone, and are likely to undergo still further. The experiments of the French chemists proved that the co-operation of nitrogenous food was so far necessary for the formation of fat, as that the animal system cannot possibly be maintained in its full vigour unless it receives a proper share of each variety of food. Nor did Liebig hesitate to admit that, under certain conditions, nitrogenous food likewise may be converted into fat; and he recalls the easy transformation, without the organism, of fibrin, when undergoing putrefaction, into ammonia and the butyric and valeric acids. Some modern physiologists, as is well known, go even a step further, by assuming that animal fat is entirely derived from nitrogenous food, the carbo-hydrates being exclusively employed for the purposes of respiration. Thus it is seen that the discussion on some secondary points of the question is even now going on; but the main point of Liebig's position, viz., that the fat originates within the animal organism, is no longer doubted by any physiologist.

In rapidly reviewing the general outcome of Liebig's labours in physiological chemistry, I cannot do more than allude, in the most cursory manner, to his more special inquiries in this department. His examination of blood; his long-continued study of the nature of bile, supported by a splendid series of analytical experiments of several of his pupils; his researches regarding the constitution of the urine of man and of the carnivora, proving the non-existence in it of lactic acid, elucidating for the first time the relation observed between the reaction—whether alkaline or acid—of the urine, and the nature of the food consumed, and leading him finally to the elaboration of his method of determining urea; and his ever-memorable investigation, in conjunction with Wöhler, of uric acid,—these researches will ever remain models unsurpassed of experimental inquiry. I must not, however, conclude this account of Liebig's chemico-physiological work without dwelling for a moment on his celebrated memoir on the constituents of the juice of flesh,* teeming, as it does, with

* The memoir has been separately published in Germany

brilliant discoveries, and presenting, even in greater abundance perhaps than any of his other papers, that inexhaustible outflow of philosophical inferences, which the fertility of Liebig's mind never failed to elicit from his discoveries. It is to his indefatigable spirit of investigation that we are indebted for the first elaborate analysis of the saline matter in the juice of flesh, the importance of whose mineral constituents was scarcely, and is not perhaps even now, sufficiently appreciated. Of the nature also of the organic substances which are present in this juice, very little was known when Liebig undertook their study. Nearly twenty years had elapsed since Chevreul had pointed out the existence of creatine in flesh, but the remarkable discovery of the illustrious French chemist had not been followed up. Even the composition of creatine had remained unknown. Liebig established its formula, and examined its products of decomposition, creatinine, sarcosine, &c., as well as many other substances occurring in the juice of flesh, which obviously play an important part in the animal economy; and he thus opened a new lode in organic chemistry, which has since been further extended, and is still being actively worked by many ardent explorers.

From tracing Liebig's course through the philo-

under the title, *Untersuchungen über das Fleisch und seine Zubereitung zum Nahrungsmittel. Heidelberg*, 1847. The English translation is entitled, Researches on the Chemistry of Food. By Justus Liebig, M.D. Edited from the manuscript of the Author by William Gregory, M.D. London, 1847.

sophical heights of natural inquiry, we may feel it perhaps a vast descent to touch on the collateral improvements of material arts and industries that were evolved from his purely scientific labours. But I should be negligent in passing unnoticed, that, exactly as his researches into the nature of plant-nutrition and the conditions of economical crop-feeding, or agriculture, gave rise to the first conception of that great industry, the manufacture of chemical manures, so also have his inquiries in animal chemistry, and more especially his investigation of the composition and nutrition of the animal body, not been long without their incidental practical outgrowth.

Who but knows that it was from Liebig's mouth that our housewives first learnt how to render the full nutritive value of meat available, or how to prepare a broth for invalids, combining the maximum of nourishing effect with the highest degree of digestibility? Who but has heard, that, having carefully studied the nature of woman's milk, he was led to compound his infant food as a substitute for mother's milk, thus becoming the benefactor even of future generations? Who, lastly, is ignorant that it is owing to Liebig's researches in physiological chemistry, that the superabundant animal nourishment of the more thinly-peopled quarters of the globe has been rendered accessible to the over-crowded populations of the opposite moiety; and that a grand commercial movement, uniting, as it were, by new bonds the two hemispheres, has been created by the organisation of a food industry already colossal, and tending still to expand with incalculable advantage to the inhabitants of Europe, thus lifted beyond the sharp pressure of deficient nourishment, and secured in the abundant supplies of those invigorating food-constituents upon which the bodily and mental energies are alike dependent for their development?

It would be difficult to quote an example of a new article having been more rapidly and universally received by European society than Liebig's extract of meat. And this is the more to be wondered at since the most opposite views prevailed, and are perhaps still prevailing, regarding its action on the organism. For whilst those were not wanting who, from the large amount of potash-salts present in it, declared this extract to be an absolute poison, the ignorance of Liebig's blind admirers, or the selfishness of interested speculators, did not hesitate to recommend it as a true substitute for meat. It is strange how such an opinion could ever have taken root, since the very mode of preparing the extract, —the careful separation of the albuminous principles, of gelatin and of fat,—sufficiently shows that at best it represents only part of the meat. Liebig's aim, in introducing his extract, was to present in it those meat-constituents which, added in varying proportions to vegetal food of various kinds, would confer upon this food in a measure the value of meat, by rendering its composition similar to that of meat.

The true mode of action of extract of meat is not as yet perfectly understood; it remains uncertain whether, as many believe, its effect is due to agents of digestion which it contains, or to its mineral saline constituents, or to creatine and the other nitrogenous principles present in it. Probably they have all a share in its action; but, in whatever way it works, the best proof of its efficacy certainly is the eagerness with which, in a comparatively short time, it has been everywhere adopted, and which, at no distant period, will give to extract of meat a diffusion equal to that of tea or coffee, or fermented liquids.

The examples I have quoted must have convinced you how varied have been Liebig's contributions to physiological chemistry, and how many the practical advantages that have flowed, incidentally as it were, and by side-long streams, from the fountain of his mighty mind. But, thankfully as we acknowledge the services actually performed, our debt of gratitude to him is equally great,—I had almost said greater still, for the impulse which his teaching has exerted upon the course of investigation amongst his contemporaries, and the guidance his genius has impressed upon the progress of discovery ever since.

I have dwelt at some length on Liebig's chemico-physiological work, longer, perhaps, than the legitimate boundaries of this lecture appear to admit. But engrossed as we are with our own small pursuits, we but too often lose sight of the giants on whose shoulders we are standing. The conviction of the powerful impetus given to agriculture by Liebig has fairly taken hold of the public mind; but his labours in the cause of physiology have not won him anything like the full meed of recognition which that part of his life-work has so nobly earned.

Reproachfully, but justly, Bischoff, in the treatise already quoted, exclaims:—"I do not believe myself mistaken if I hold the opinion that there are not many among the younger generation of physiologists and medical men who know, or have even a distant notion, how great, I should rather say how immense, the influence of Liebig's researches, of his writings and teachings, has been and is still, not only on physiology and medicine, but on organic science at large. The majority enjoy the advantages gained, and rejoice in the progress, without being conscious of the author. They consider as self-evident the facts established by Liebig, the methods and principles of research diffused by his teaching. They believe that it cannot be otherwise, and care but little for him to whom science, and with science they themselves, are indebted for their present position."

We have now, I think, made a fairly complete, though, of necessity, a cursory and brief survey of the general course and scope of Liebig's indefatigable researches; and we cannot, I feel, but be vividly impressed with the numerous and extensive fields of inquiry over which his active intellect ranged. We have found him most frequently working in the domain of pure chemistry and largely extending the list of its products and processes, the abundance of its experimental facts, the breadth of its theoretic interpretations, and the prolific forecasts afforded by its laws. Sometimes, in the course of these purely chemical researches, we have observed his experiments bearing also upon collateral sciences; and from these, in their turn, he has drawn welcome rays of light for the elucidation of his most cherished branch of research, chemistry proper. At other times we have had occasion to note with admiration how his clear mind, never too abstract in its operations to neglect any means of ameliorating the arts of life, drew even from his most abstruse discoveries the conditions of industrial improvements of the highest material value to mankind.

In many, or in all, of these widely divergent paths, the researches of Liebig, and their splendid results, theoretic and practical, would well repay our detailed examination, and would, I am sure, be of the deepest interest to every one in this hall. But within the brief space of time allotted to this discourse, we could not follow out in detail any of the wide fields laid open and fertilised by Liebig's prodigal mind; and from so many paths of illustration I must single out some one alone upon which to dwell at present; nor can the choice which it devolves on me to make be doubtful.

I feel that in fixing this choice I have to reflect, firstly, that Liebig was himself, above and before all things, a chemist; and, secondly, that the audience I have the honour to address consists, for the most part, of chemists; while, even of the non-professional friends who favour us with their company as guests, though few may perhaps be actual toilers in the laboratory, the majority are, if I may use the expression, chemists at heart, drawn hither into the society of us chemists by their scientific predilection and sympathy for our special pursuits, and eager, in union with ourselves, to pay the homage of their affectionate gratitude to the greatest chemist of our time.

Bound, therefore, as I am, in my selection, by the chain of Time's rigorous restriction, I will ask your permission to confine the remainder of this discourse to that portion of Liebig's life-work which was devoted to pure chemistry,—that noble science to which so many here assembled consecrate their lives, and of which the Institution wherein we have met is one of the most memorable temples.

In pursuing this plan, I feel that I must plead for the indulgent pardon of those of my audience who, though deeply interested in chemistry, have not made this science the subject of their special study. They will forgive me if, in reminding my brother chemists of some of Liebig's experimental inquiries, I have to make use of names and terms familiar as the alphabet to working chemists, but falling obscurely and harshly upon non-professional ears. They will, I trust, remember, in my excuse, that the unavoidable incongruities marking the composition of a general audience cannot but impart some tincture of its own diversified character to the discourse which it calls forth and inspires. And feeling, as they must, that a chemist invited by chemists to speak of the chemical work of Liebig is obliged of necessity to discourse in chemical language, they surely will not find fault with him if he allows himself, in spirit, to be transported for a while from the popular air of Albemarle Street to the sterner atmosphere of Burlington House.

But, even in the comparatively restricted range of chemistry proper, those of my auditors who know the voluminous character of Liebig's contributions to this great domain of science, will best judge to how very few among a total of researches counting by hundreds, our further illustration of Liebig's career must be confined. Indeed, let me remind you that, in the Royal Society's well-known record of scientific papers, Liebig's contributions number no fewer than 317, whereof 283 are by himself alone, and the remaining few by Liebig working in collaboration with others. The great majority of these researches relate to chemistry proper. But even this protracted list brings the enumeration of his papers only to the year 1863, *i.e.*, to the date of the Royal Society's record. From that period down to his death in 1873, Liebig, although chiefly working on collateral fields of inquiry, more especially those of agriculture and physiology, made many and most valuable additions to the series of his chemical papers. Of these papers, indeed, the mere titles would require for their perusal the whole time still at my disposal; and if, from among their number, I can give time to the detailed examination of some six or seven, I shall have made the closest approach in my power to a typical survey of Liebig's chemical labours. Yet, even with this twofold restriction of our field of choice, the utmost difficulty remains in framing any general principle sufficiently comprehensive to guide us in selecting from among the vastness and variety of philosophical wealth bequeathed by Liebig's genius for our enrichment: and I am far from confident of obtaining your unqualified approbation of the few topics which I have ultimately chosen to bring, in some degree of detail, under your notice.

And here, Ladies and Gentlemen, permit me to interrupt, for a minute or two, the thread of my discourse, that I may advert in a few grateful words to the kind assistance lent us on this occasion by my friend Mr. Herbert McLeod, in former days successively my pupil and my assistant, now the distinguished Professor of Experimental Science in the Indian College for Civil Engineering. He is with us, as you see, ready to help me in showing to you at all events some of Liebig's experiments, and, altogether forgetful of his present professorial dignities; eager and happy to contribute his mature power and experience in any function, however subordinate, feeling that his service is made honourable by the great name to whose glory it is an affectionate tribute.

Nor has the disinterested help of others been wanting on this occasion. You observe on the lecture-table a collection of the substances discovered or studied by Liebig, much more perfect and complete probably, than he himself had ever simultaneously under his eyes. The happy privilege of exhibiting to you this encyclopædic display of Liebig's products I owe to the enthusiastic ardour of some of my young friends studying in the Berlin Laboratory, and more especially to Drs. Gabriel, Jahn, Römer, Kretschmer, and Zierold, who, proud to participate in the homage we render to Liebig, have for months past devoted their time and energy to preparing and classifying these substances, with no other object than that of bringing the work of their great landsman as conspicuously as possible under the notice of the chemists of this country.

Still less can I omit to mention my gratitude to my old friend and frequent collaborator, Mr. F. O. Ward, for his invaluable assistance on this occasion. I have in early life, as some of my audience may remember, lived in this country, and the time thus passed in dear old England belongs to the brightest recollections of my existence. But a long time has elapsed since I returned to my native land, and ten years' exclusive use of my mother-tongue has not, I fear, improved my English style. Under these circumstances, I was glad to submit for revision the manuscript of this lecture to Mr. Ward, who has generously and freely lent to his German friend the ever-ready assistance of his practised skill in English composition. I have particular pleasure in adding that Mr. Ward was happy and proud, as we all of us here are, I am sure, to contribute a few stones to the edifice we are endeavouring to raise to Faraday's memory, and to mark with his imperishable name.

Now,—to take up again the thread we have dropped,—of the few portions of Liebig's work which I intend to submit somewhat more in detail to your notice, I must first advert to that which, whether or not the most brilliant emanation of his inventive powers, is certainly that which has conduced, more than any other of his great discoveries, to facilitate the productive labours of the chemical community, and has been the main source of that marvellous development of chemistry, especially organic, which will be looked back to hereafter as one of the chief glories of our age. I allude, as many present have doubtless already divined, to Liebig's APPARATUS, in its action so perfect, in its simplicity so beautiful, for the ANALYSIS BY COMBUS-

TION OF ORGANIC BODIES, and more especially for the determination of their carbon by ponderal, instead of, as in the old method, by volumetric measurement. Many an eye here has already glanced at Liebig's combustion apparatus, which, in its simplest form, I have placed on the table before you. Need I say that to the valuable instrumentality of this apparatus, with its celebrated five-bulb appendage—that to the aid of this familiar companion, and partner, and mitigator of his toils—the working chemist recognises himself indebted for his best successes in analytical researches within the domain of organic chemistry?

The present generation of chemists have not the most remote idea of the difficulties which attended an organic analysis before Liebig invented his bulb-apparatus. It is, indeed, almost impossible to conceive ourselves transported to the time in which this apparatus did not exist. At all events, I have no fear of contradiction when I say that at present more organic analyses are made in a single day than were accomplished before Liebig's time in a whole year; and that, if the period of a man's life has sufficed to rear the now proud structure of organic chemistry, it is by means of Liebig's apparatus that this great result has been achieved. It is the extraordinary simplicity of the instrument which constitutes its great merit. Many have attempted to improve it, but have only complicated it, and chemists have almost always returned to the old form. Liebig employed charcoal; we use, naturally, the more convenient form of fuel which the marvellous development of the industry of coal-gas has placed at our disposal; but, in other respects, in the majority of laboratories, the method is used precisely as Liebig presented it to science nearly half a century ago.*

The combustion of an organic substance teaches us its percentage composition. There still remains its molecular weight to be determined. For acids the method best suited for the purpose has long been known. For bases Liebig devised, and employed with perfect success, a peculiarly bent tube, in which the substance was weighed and, after having been exposed to an atmosphere of chlorhydric acid, re-weighed. In the present day this apparatus is no longer employed for its original purpose, since Liebig himself discovered a more elegant and, at the same time, more acccurate method of procedure; but as the simplest and most trustworthy of all forms of desiccating apparatus,—for which purpose he also proposed it,—it still remains one of our most useful

* Liebig's papers on Organic Analysis appeared in the *Annales de Chimie et de Physique*, and in his own Journal. A description of his methods was also separately published in Germany under the title, *Anleitung zur Analyse organischer Körper*. 2*te Aufl.* Braunschweig, 1853. The English edition is entitled, "Handbook of Organic Analysis; containing a detailed Account of the various Methods used in determining the Elementary Composition of Organic Substances." By Justus von Liebig, Professor of Chemistry in Munich. Edited by A. W. Hofmann, Professor in the Royal College of Chemistry. London, 1853.

instruments, and one most universally employed in all laboratories.

But what was the method of ascertaining the molecular weight of a base, which induced Liebig to abandon his original process known as the chlorhydric acid process? Among the chemists here assembled, there is certainly not one who has not often determined the molecular weight of bases by the analysis of their platinum-compounds, but there may be many present who are not aware that for this most accurate process we are also indebted to Liebig. For my own part I confess that, although I have used the method very frequently, I was not cognisant of the fact as to its origin, until I learnt it whilst carefully examining Liebig's memoirs during the past winter. And this is not to be wondered at, for in reality Liebig never published any particular paper on the subject, but merely mentions the method incidentally, as it were, in a notice of Regnault's researches on organic bases. Permit me to remind you of the elegance of this process by an experiment. The four test-glasses before us contain solutions of the chlorhydrates of morphine, quinine, cinchonine, and narcotine; and the yellow precipitates which are thrown down by adding a solution of platinic chloride are the first platinum-compounds ever formed for the purpose of fixing the molecular weights of organic bases by the platinum process.

The distinguishing characteristic of the methods introduced by Liebig is simplicity. And this merit also belongs, in an eminent degree, to his process of air-analysis. That an alkaline solution of pyrogallic acid takes up oxygen and becomes progressively more and more blackened by its absorption, had long been known as a bare fact; but it was reserved for Liebig to found on this fact a method for the estimation of oxygen, and that method one surpassing all others in the facility of its execution. Liebig indeed showed—and the experiment we are performing proves it to you—that such a solution absorbs oxygen with nearly as much avidity as potash displays in absorbing carbonic acid. And the necessity of an alkali in the reaction further suggested at once to Liebig the idea of combining the estimation of carbonic acid with that of oxygen, by treating the gas to be examined first with potash and then with pyrogallic acid.

I might mention here many other methods and forms of apparatus bequeathed by this great inventor for our use, but I shall content myself with drawing your attention to one more contrivance only,—a contrivance most extensively employed, not only in scientific laboratories, but also in chemical manufactories. I refer to the well-known "Liebig's condenser," which you see in various forms on the table. And of this most useful apparatus, which we are daily using, Liebig has never given a special account; he only mentions it incidentally in one of his early papers on the action of chlorine on alcohol.

When we pass from the analytical methods and apparatus devised by Liebig to the researches which he effected by their instrumentality, we find among the first to claim our attention his investigations in the CYANOGEN GROUP.

These investigations are the necessary offspring of his experiments on the fulminates, which lead us back to the earliest stages of his scientific career. Indeed the first paper in which Liebig appears before the public bears the title: "Some remarks on the preparation and composition of Brugnatelli's and Howard's fulminating mercury."*

This research dates from the year 1822, when, after a residence of several terms in the University of Bonn, he obtained the degree of doctor of philosophy in the University of Erlangen. Liebig was then 19 years old,—he was born on May 12, 1803—and as in this paper we read of experiments extending over a period of two years, we have convincing proof how early he recognised the natural bent of his mind, and devoted himself to its development.

This first paper of Liebig, although remarkable for the clearness and precision with which he describes his experiments, is very far from elucidating the nature of the fulminates in a final and satisfactory manner. But, considering the unfavourable

* *Einige Bemerkungen über die Bereitung und Zusammensetzung des Brugnatelli'schen und Howard'schen Knallquecksilbers. Von Herrn Liebig, der Chemie Beflissenen aus Darmstadt.* Kastner's *Repert. f. Pharm.*, xii, 412.

conditions under which the inquiry was made, it may be looked upon as holding out good promise of what its author, under better circumstances, might be expected to achieve.

In the first quarter of the present century, the opportunities in Germany for instruction in experimental science were very scanty. Liebig's most earnest wish, therefore, was to continue his studies in Paris, at that time the centre of chemical and physical investigation, where with Gay-Lussac, Thenard, Chevreul, Dulong, Biot, Ampère, and Savart, the most celebrated representatives of chemistry and physics, he might fairly hope to satisfy his thirst for scientific knowledge. And although so young, he must have already attracted the attention of influential persons, since he had the rare good fortune of receiving from the then reigning Grand Duke of Hesse-Darmstadt, Louis the First, the means for a residence of several years in Paris.

The year 1823 finds the young doctor in the metropolis on the Seine. Without introductions to any of the *coryphées* of science, it is difficult for a stranger to gain admittance into a chemical laboratory. Finally he succeeds, however, on the recommendation of Thenard, in finding an opportunity of resuming, in the laboratory of Gaultier de Claubry, his researches begun in Germany upon the explosive compounds. The results of this investigation are published in the 24th vol. of the *Annales de Chimie et de Physique.*

Liebig himself, however, has no confidence in these results. The method of analysis employed is not sufficiently exact, and new experiments are therefore necessary to establish the composition of the fulminating bodies.

By a fortunate occurrence, he is soon enabled to resume these researches under more favourable auspices. The young investigator makes the acquaintance of Alexander von Humboldt, whose interest obtains for him the cherished wish of his life, admittance into Gay-Lussac's laboratory.

The influence which these new associations exercised on Liebig's scientific career is so striking that I cannot refrain from quoting the words in which he describes his first meeting with Alexander von Humboldt, in the dedication to that great naturalist of his work on Chemistry in its Applications to Agriculture and Physiology:—"During my stay in Paris, in the summer of 1823, I succeeded in presenting to the Royal Academy an analytical investigation of Howard's fulminating compounds of silver and mercury, my first inquiry. At the end of the meeting of July 28th I was engaged in packing up my specimens, when a gentleman left the ranks of the Academicians and entered into conversation with me. With the most winning affability he asked me about my studies, occupations, and plans. We separated before my embarrassment and shyness had allowed me to ask who had taken so kind an interest in me. This conversation became the corner-stone of my future. I had gained the most amiable friend, the most powerful patron of my scientific pursuits.

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"Unknown, without introductions in a city where the assemblage of so many men from every quarter of the globe is the greatest hindrance to personal acquaintance with scientific men high in renown, I might, like so many others, have remained unnoticed—perhaps have failed altogether; this danger was now entirely averted. From this day forth I found all doors, all institutes, and laboratories open to me. The lively interest you took in me procured me the affection and intimate friendship of my dear teachers, Gay-Lussac, Dulong, and Thenard. Your confidence paved the way to my present sphere of action, which for sixteen years with unabated zeal I have striven worthily to fill."

How fortunate Humboldt's recommendation to Gay-Lussac proved, and how soon the latter recognised the ardour and ability of his *protégé*, may be sufficiently appreciated from the readiness with which the older chemist took part in the inquiry already begun by the younger. On the other hand, it is only necessary to compare the results of these joint labours with the results previously published by Liebig alone, to see how much the young German chemist owed to his French teacher, who was soon to become his paternal friend.

As early as on the 24th of March of the following year (1824) it was possible to present the new analysis of fulminating silver to the French Academy; the paper is printed in the 28th volume of the *Annales de Chimie et de Physique*, under the title, "*Analyse du fulminate d'argent, par MM. Liebig et Gay-Lussac.*" In this paper they develop the formula for fulminating silver which is in use at the present day.

I am not permitted to follow them into the details of this memorable inquiry, but I cannot refrain from alluding to the interesting fact that even at that remote period Gay-Lussac and Liebig trace an analogy between fulminic and picric acid (Welter's bitter as the latter compound was then called),—a view to which our modern conceptions have returned, more especially since we know that the action of chlorine upon both compounds gives rise to the formation of the same substance, Stenhouse's chloropicrin.

The researches carried out by Liebig, in conjunction with Gay-Lussac, exercised a powerful influence on the career of the young chemist. With increased confidence in his own resources, he learned how great is the saving of time and labour when the beginner has the good fortune to thread the labyrinth of chemical phenomena under the conduct of a trustworthy guide. It was in Gay-Lussac's laboratory that Liebig conceived the idea of founding in Germany a chemical school, where he hoped to be to his younger fellow-workers what Gay-Lussac had been to him.

How gloriously this ambitious dream of his youth has been fulfilled! In the same year, once more on the recommendation of Humboldt, he was appointed Professor of Chemistry in the little Hessian University on the banks of the Lahn; and, ever keeping before his eyes the great goal, from the most modest beginning and with the scantiest means, he succeeded in establishing a school whose wonderful achievements fill one of the most glorious pages in the early history of organic chemistry.

The experiments made in conjunction with Gay-Lussac having proved the fulminates to possess the same composition as the cyanates,—to be isomeric with them, as we should now say,—Liebig himself was induced to engage in the study of this prolific group of compounds. In this study he had been preceded by Wöhler, and it is characteristic of the two men that this meeting upon the same ground,—but too often the cause of misunderstandings, if not of worse, among investigators—was the origin of an intimate and lifelong friendship, soon to exercise a most important influence upon the development of organic chemistry. But a few years later the two friends had become ardent fellow-workers; and ever since, the light emanating from the associated inquirers, *dioscuri* in the chemical firmament, has been the beacon to which we look for guidance in our researches.

The raw material for the production of the cyanides has been, at all times, the yellow prussiate of potash; but, although this compound has been manufactured on a large scale for a great many years, no satisfactory explanation of the process of fusing animal matter with potash had ever been offered before Liebig's profound investigation of the subject showed that the product first formed is only potassic cyanide, and that it is by the taking up of iron from the retort, or otherwise, during the process of lixiviation, that the cyanide is converted into ferrocyanide. The present mode of manufacturing PRUSSIATES is based in a great measure on the knowledge derived from this investigation.

The recognition of these facts led Liebig naturally into a more minute study of ferrocyanogen-compounds in general, and among the various valuable results ensuing from this inquiry we may specially single out for notice his simple method of producing FERROCYANIC ACID—admitting as it does of an easy experimental illustration. By mixing a solution of the yellow prussiate with chlorhydric acid and ether, you observe the compound separating in the form of a white felted mass, which, by washing with ether, is easily obtained in a state of purity.

Another valuable point elicited by Liebig in the course of these researches was his elegant process for obtaining POTASSIC CYANIDE by fusing the ferrocyanide with potassic carbonate. This cyanide formerly had been mostly prepared by passing cyanhydric acid through an alcoholic potash-solution, and had therefore found but few applications. At present this salt, so extensively used in electro-plating and photography, is manufactured on the large scale invariably by Liebig's method.

The facilitated production of this important body gave rise to the rapid development of many other interesting applications. Liebig recognised in potassic cyanide one of the most important enrichments of analytical chemistry. There is no more powerful reducing agent known than this cyanide. Many metallic oxides and sulphides are instantly reduced by it. We have before us a Florence flask containing fused potassic cyanide, to which we now add red oxide of lead. The red colour immediately disappears, and metallic lead makes its appearance in the form of a finely-divided grey precipitate which, when heated for some time, collects to a regulus; whilst on the surface floats a transparent layer of fused saline matter which, on cooling, solidifies to a white cake. Some time being required for the subsidence of the metal, we have made a few experiments before the lecture, and you observe in the several flasks upon the table how sharply the two layers have become divided. The porcelain-like mass, which is easily separated from the lead, consists of potassic cyanate, and thus this process affords a ready mode of obtaining on the one hand a reduced metallic product, and on the other, a CYANATE.

The easy production of cyanates suggested at once to Liebig's fertile mind that simple and elegant process by which we now prepare UREA. I cannot deny myself the pleasure of illustrating this process by an experiment, affording me, as it does, at the same time, an opportunity of exhibiting to you, in its simplest form, Wöhler's classical synthesis of urea, to which I have already alluded when speaking of Liebig's physiological work in a previous part of this lecture. The two beakers before us contain cold saturated solutions of potassic cyanate and ammonic sulphate in equivalent proportions. On mixing the two solutions you observe the immediate formation of a white crystalline precipitate. This precipitate, consisting of potassic sulphate, we separate by filtration, and obtain a clear solution of the normal ammonic cyanate, which we divide into two parts. Of these two solutions, the one is boiled for a few minutes, whereby the normal cyanate is converted into urea. Addition of concentrated nitric acid enables us to recognise this conversion, for it produces in the liquid a crystalline precipitate, very much increased by cooling, of urea nitrate; whilst the liquid containing the unconverted cyanate is decomposed, with evolution of carbonic acid.

From the cyanates there is but one step to the SULPHOCYANATES.

This name will recall to every chemist here, I feel assured, one of the most interesting of the many series of substances with which the indefatigable toils of Liebig have enriched our science. It would be vain to attempt anything like an encyclopædic account of the splendid chain of compounds derived from ammonium sulphocyanate: to exemplify the features of this chapter of Liebig's work, we must be satisfied to glance rapidly at one or two groups of these bodies; and I will select for this purpose, with your permission, the bases of the MELAMINE SERIES and the MELLONIDES. Our reference to these bodies, brief as it must be, will have for us an additional interest, as it will of necessity recall to our attention the origin and character of the memorable contest which arose concerning these compounds between Liebig, on the one side, and Gerhardt and, in a measure, Laurent, on the other.

Scientific history affords few examples of a struggle so vehement, and often so impetuous, waged between combatants so eminent; and if sometimes an excess of zeal led on both sides to expressions overcharged, or trespassing beyond the due boundaries of scientific calmness, the conflict between old established views and new conceptions, and between experimental facts and theoretical interpretations apparently incompatible, naturally resulted in a most valuable sifting of corn from chaff. From such an encounter between such champions, truths profound and novel could not fail to be elicited in the end: while, on the other hand, it would be difficult to cite any instance more strikingly illustrative of the pangs so often attendant upon the birth of truth. On now looking back to this long bygone struggle, we cannot but regret that the dispute should not have been brought to a settlement more easily and more early, so as to have consumed less of the precious time and intellectual power of these three great men; especially as we perceive that they had, each and all of them, but one equal aim at truth, and that after all it was rather as to the road by which truth should be sought, and the method by which it should be worked out, than as to the truth itself, that their main conflict arose.

The starting point of their controversy originated in the difference of their respective views as to the action of heat upon the ammonium compound of sulphocyanic acid. When heated, this salt leaves as a residue an amorphous body, by Liebig denominated melam. His experiments upon this body were attended with results quite unexpected, and in those days unprecedented. Nothing had previously been observed in any degree resembling the series of remarkable transformations which melam underwent when treated with alkalis or acids. Liebig's object in studying the action of heat upon the ammonium sulphocyanate had been to split up further a comparatively simple compound, and his astonishment may well be conceived on finding that the treatment he had adopted to this end gave rise, on the contrary, to more and more complex organic bodies; so that at one time he was actually led to indulge in the hope of being not far from the artificial production of uric acid itself. Molecular condensations of this description, though of daily occurrence in the reactions we at present study, had, at the period referred to, scarcely ever been met with : hence the wonder and exultation with which Liebig contemplated the new facts he had just ascertained.

The formula of melam, the product of the molecular condensation of the ammonium sulphocyanate, is $C_6H_9N_{11}$, and when this body is boiled with potash it takes up the elements of one molecule of water, and splits into melamine, $C_3H_6N_6$, and ammeline, $C_3H_5N_5O$. Nitric and sulphuric acids convert melam, melamine, and ammeline in the first place into ammelide, $C_6H_9N_9O_3$, and finally, by protracted ebullition, into cyanuric acid, $C_3H_3N_3O_3$. In this series, a link of transition, the compound $C_3H_4N_4O_2$ is wanting. The gap is, however, soon filled up by Liebig and Wöhler's discovery of melanurenic acid among the products of the action of heat upon urea. The same reaction prevails throughout the several stages of this transition; the elements of water are assimilated while ammonia is evolved; or, to use the language of the day, the hydroxyl group is substituted for amidogen, as a glance at the following table shows :—

Melamine	$C_3H_6N_6$	=	$(C_3N_3)\ (NH_2)_3$
Ammeline	$C_3H_5N_5O$	=	$(C_3N_3)\ (NH_2)_2(OH)$
Ammelide	$\left\{\begin{matrix} C_3H_5N_5O \\ C_3H_4N_4O_2 \end{matrix}\right\}$	=	$(C_3N_3)_2(NH_2)_3(OH)_3$
Melanurenic acid ..	$C_3H_4N_4O_2$	=	$(C_3N_3)\ (NH_2)\ (OH)_2$
Cyanuric acid	$C_3H_3N_3O_3$	=	$(C_3N_3)\ \ (OH)_3$

To this change in the composition of the substances corresponds the gradual alteration of their chemical character. Melamine is a powerful base, forming well-defined salts; ammeline is still basic, but much less distinctly so; the two following terms are at once basic and acid; while cyanuric acid, lastly, is a powerful acid.

At this point we face the source of Liebig's contest with Gerhardt and Laurent; and we perceive at once the peculiar character of Liebig's experimental tendencies in striking contrast with his antagonists' speculative predilections.

Liebig, in his first memoir on the series of compounds in question, mentions only ammeline and ammelide as intermediate terms between melamine and cyanuric acid. Gerhardt's penetrating intellect forecasts the existence of an additional body, of the term $C_3H_4N_4O_2$; but instead of proving the truth of his speculation by experiments of his own, he is satisfied to declare Liebig's investigation inexact, and to deny that the compound named ammelide by Liebig possesses the composition assigned to it by its discoverer, this compound, he goes on to assert, being the very body he has been led to preconceive.

Against this theoretic attack upon a fact experimentally established by his own, and unequivocally confirmed, moreover, by Knapp's subsequent researches, Liebig enters his solemn protest, entirely denying his antagonist's right to attempt the overthrow of a formula analytically worked out, by arbitrarily substituting for it the mere imaginative conception of a substance as yet unproduced. He declares that a scientific disputant adopting such a mode of procedure places himself in direct opposition to the sound principles of all natural inquiry. And, as if to dash into fragments his adversary's argument by the sledge hammer of a fact,—ever his favourite weapon in scientific warfare—he produces, in collaboration with Wöhler, the very compound which had been floating in Gerhardt's imagination, and shows that the properties actually belonging to Gerhardt's theoretic compound were entirely different from those of his own experimentally discovered body, ammelide, so that, had Gerhardt worked, as a chemist should have done, by experiment, he would have wrought out his refutation with his own hands. Without entering into details as to the experimental means employed by Liebig and Wöhler on this occasion, it is sufficient to say, as we have briefly noted before, that they found the body in question—Gerhardt's dream and their realised fact—among the products of the decomposition of urea by the action of heat.

The justice of Liebig's remonstrance against Gerhardt's impugnment of the former's experimental results by aid of a mere visionary theory, has derived ample confirmation from chemical researches subsequently made. For it is now a fact established by indisputable experiment, that the decomposition of melam yields, as one of its products, a substance having the composition and properties assigned by Liebig to ammelide. On the other hand, it is but justice to Gerhardt to state here frankly that experimental researches have also fully borne out Gerhardt's theoretical anticipation, seeing that the urea derivative, called by Liebig and Wöhler melanurenic acid, is now proved to be also obtainable by proper treatment from melam.

Of a very similar character is the collateral controversy regarding the nature of the mellonides. On heating melam, Liebig obtains an amorphous body of variable composition, which he calls mellon. This body, when fused either with metallic potassium or potassic sulphocyanate, furnishes a beautiful saline compound, crystallising in slender needles, potassic mellonide, KC_3N_4. This formula does not agree with Gerhardt's view of the nature of the compound; it does not comply with certain empirical rules which Gerhardt and Laurent have deduced from the examination of numerous substances. A remedy is found. Gerhardt substitutes for the expression KC_3N_4 the formula $K_2H_2C_6N_8O$, assuming the existence of hydrogen and oxygen in the salt, which, he suggests, Liebig must have overlooked. Again, Liebig, in language not to be mistaken, denounces this illegitimate tampering with the results of experiment. He points out that analysis has furnished neither hydrogen nor oxygen,—nay, that the substance, from the very conditions under which it is formed, cannot contain these elements. A careful repetition of the experiments unequivocally confirms their absence, clearly proving Gerhardt's assumption to be erroneous. But in this case, likewise, Gerhardt's speculation has not altogether deceived him. The formula originally assigned by Liebig to potassic mellonide is, indeed, not absolutely correct, as he soon afterwards himself points out. Experiments, made at his suggestion by Henneberg, show that the mellonides, when fused with potash, are converted into a peculiar compound, cyameluric acid, the composition of which is not reconcilable with the old formula of potassic mellonide, and lead Liebig to adopt for this latter the expression $K_3C_9N_{13}$, scarcely different from the treble of the formula originally proposed. Singularly enough, the new formula agrees also with Gerhardt and Laurent's rule, and the hot contest was thus brought to a conclusion satisfactory to all parties.

The brief sketch I have given of the principal incidents of this remarkable controversy shows us Liebig ever anxious to retain the safe guidance of experiment. A happy speculator himself, he never fails to curb the evolutions of his imagination by the bridle of sober observation. If he finds his speculation to be in contradiction with recognised facts, he endeavours to set these facts aside by new experiments, and failing to do so, he drops the speculation. Nothing is less sympathetic to Liebig than any attempt to overrule analytical discrepancies by conclusions drawn from analogy, to reconcile discordant observations by sweeping assumptions, or to settle disputed questions by speculative conceptions alone. "In organic chemistry," he says,* "it is indispensably necessary to leave to phenomena not yet understood the undiminished charm of being unexplained. If experiments be required for this explanation, any anticipated theory will deter chemists from undertaking them; and this the more so, the more perfect and rounded off the form in which such theory presents itself. No one can feel inclined to search for a key if there be reason to believe that the result of all his search will be to prove that another man has got the key already in his pocket. And yet the explanation will be permanently acquired for science only by him who actually undertakes the experiments."

Indeed, during his long contest with Gerhardt, Liebig never loses the sure foundation of experiment; he has not to retract a single statement as to facts, though he does not deny having committed mistakes in interpretation; but these he happily compares to the broken crockery found in the corners of the best regulated houses in which a good deal of work has been going on.

Indeed, in the early part of his scientific career, Liebig had paid dearly for not having followed the rule which, ever afterwards, he so strenuously inculcated. Speculating without experimenting cost him

* *Ueber die Constitution der Mellonverbindungen.* Ann. Chem. Pharm., lxxxv, 263.

the discovery of bromine. Many years later, on the occasion of another discussion with Laurent, Liebig, with characteristic frankness, told us the story himself. It is so instructive that I give it to you in his own words.

"No greater misfortune," he says,* "can befall a chemist than being unable to disengage himself from preconceived ideas, and yielding to the bias of his mind to account for all phenomena, not agreeing with his conceptions, by explanations not founded on experiment. This generally happens with persons who possess but little experience in chemical investigations. Such cases are of daily occurrence in the laboratory. If to a beginner in analysis I give a mineral with the remark that he must look for antimony, lead, and potassium, I am sure he will find antimony, lead, and potassium in spite of all contradictory evidence, for he will contrive an explanation satisfactory to himself of every discrepant reaction.

"I know a chemist who, while at Kreuznach, many years ago, undertook an investigation of the mother-liquor from the salt works. He found iodine in it; he observed, moreover, that the iodide of starch turned of a fiery yellow by standing overnight. The phenomenon struck him; he procured a large quantity of the mother-liquor, saturated it with chlorine, and obtained by distillation a considerable amount of a liquid colouring starch yellow, and possessing the

* *Ueber Laurent's Theorie der organischen Verbindungen.* Ann. Chem. Pharm., xxv, 29.

external properties of chloride of iodine, but differing in many of its reactions from the latter compound. He explained, however, every discrepancy most satisfactorily to himself; he contrived for himself a theory on it (*er machte sich eine Theorie darüber*).

"Several months later, he received the splendid paper of M. Balard, and, on the very same day, he was in a condition to publish a series of experiments on the behaviour of bromine with iron, platinum, and carbon, for Balard's bromine stood in his laboratory, labelled Liquid chloride of iodine. Since that time, he makes no more theories unless they are supported and confirmed by unequivocal experiments; and I can positively assert that he has not fared badly by so doing."

We have almost lost sight of the group of compounds from which Liebig's controversy with the French chemists arose, viz., the sulphocyanates. Nor need they claim our attention much longer. I cannot, however, take leave of these compounds without briefly alluding to an interesting collateral result which emanated from this inquiry. In the course of his experiments, Liebig consumed a large amount of ammonium sulphocyanate, and was thus naturally led to search for a simple method of obtaining this salt, which he ultimately found to consist in the treatment of cyanhydric acid with yellow ammonium sulphide. In studying the reaction of these two compounds with a view to the production of ammonium sulphocyanate, Liebig at once perceived an analytical advantage derivable therefrom, viz., the delicate test which it affords for prussic acid. The experiment is easily made. We have before us a dilute solution of cyanhydric acid, which we mix with a few drops of yellow ammonium sulphide. The liquid is now boiled till the excess of sulphide is driven off; already lead paper is no longer blackened by the steam evolved. The solution now contains pure ammonium sulphocyanate, and a drop of ferric chloride produces the well-known blood-red colour. The evidence thus obtained of the existence of a sulphocyanate affords us unequivocal proof of the original presence of prussic acid in the solution.

I have dwelt at some length on the investigation of the derivatives of the sulphocyanates, and on Liebig's controversies with Gerhardt and Laurent, not so much on account of the interest attached to the subject, great as it is, but rather because they enabled me to exhibit to you the essential character of Liebig's mode of thinking and working, which, in a great measure, was also that of Faraday. How much these two great men resembled each other in this respect, and how much, therefore, they must have felt an interest in each other, is clearly seen from a passage, in which Liebig speaks of Faraday, and which, although it is in no way connected with the subject we have been investigating, I cannot deny myself the pleasure of reading to you. In an Academical speech on Induction and Deduction,* Liebig says :—

* *Induction und Deduction. Akademische Rede, geh. am* 28 *Mai*, 1865. *Reden u. Abhandlungen.* S. 296.

"In one of Faraday's grandest discoveries, we find another example of compound induction.

"Oersted was the first to generate magnetism in metallic rods by an electric current.

"Faraday, on the other hand, conversely endeavoured to produce a spark or current by means of a magnet. His object was to elicit a phenomenon, and since the law governing this phenomenon, and the mode of eliciting it were not known, the problem could be solved only by an artistic process, by the inductive method. The phenomenon once known in its various relations, was now capable of becoming the object of a deductive investigation, and the antithesis of Faraday's inductive and Weber's deductive inquiry is thus rendered obvious. Faraday sought for the *thing*, if this expression be permitted, Weber for the *cause*, for the *law*. I have heard mathematical physicists deplore that Faraday's records of his labours were difficult to read and understand, that they often resembled rather abstracts from a diary. But the fault was theirs, not Faraday's; to physicists, who have approached physics by the road of chemistry, Faraday's memoirs sound like admirably beautiful music."

Nor, let me parenthetically here remark, was Liebig's admiration confined to Faraday's researches; it was as warmly felt for Faraday's character. It is indeed delightful to think that two men, so fully qualified to appreciate each other, lived for years on terms of cordial friendship, exchanging from time to time tokens of good will and regard. It was in 1844 that Liebig, visiting England for the first time, became acquainted with Faraday, and I am indebted to Mrs. Faraday for a letter addressed by him to her late husband, written soon after his return to Giessen, which admirably reflects the deep impression he had carried away.* In a later letter, Liebig thanks Faraday for the friendly interest he has taken in obtaining permission for his son to enter the medical service of the Indian Army. On examining the voluminous correspondence left by Liebig, letters of a similar character written by Faraday cannot fail to be forthcoming.

Some of the happiest experimental inquiries of Liebig were instituted by him in conjunction with his intimate friend, Friedrich Wöhler. Their joint investigation of the GROUP OF BENZOIC COMPOUNDS must undoubtedly be considered one of the most beautiful and fruitful results of these associated labours. How vast and far-reaching has been the influence of this inquiry in shaping our conceptions as to the general nature of chemical compounds! Is it not here that we first encounter some of those grand reactions by which—as by so many landmarks—the investigator has ever since steered his course on the troubled sea of chemical phenomena? What a flood of light has poured into

* A heliotype of this letter, by Messrs. Burchard brothers of Berlin, is appended to this lecture.

science simply from the development of thoughts propounded in this inquiry! Must we not behold in it the first foundations of the gigantic structure of organic chemistry we admire at the present time? Have not Liebig and Wöhler drawn from this source the prominent and typical members of the group of aromatic compounds which, as Kekulé has taught us, radiate from Benzol as from a central star, unlimited in number and variety?

But let us rapidly review the principal points of this magnificent inquiry.

When the associated investigators (1832) take the field, oil of bitter almonds and benzoic acid are already known, as is also the conversion of the former into the latter by the action of atmospheric air. Nothing more, however, has as yet been made out.

Liebig and Wöhler, in the first place, analyse oil of bitter almonds, and establish the formula still in use. But the formula of benzoic acid then accepted, which Berzelius had deduced from his analysis of the lead salt, cannot be reconciled with this expression. Hence the necessity of repeating the investigation of this acid. Liebig and Wöhler, by analysing silver benzoate, arrive at the formula thenceforward recognised as the true one, and the relation between oil of bitter almonds and benzoic acid is, from this moment, elucidated for all time. Oil of bitter almonds is shown by them to be a compound of the ternary radicle Benzoyl with hydrogen; benzoic acid, they prove to be a combination of the same radicle with a group of oxygen and hydrogen atoms, considered by Liebig and Wöhler to consist of oxygen and water, but which, at the present time, we term hydroxyl.

By treating oil of bitter almonds with chlorine, Liebig and Wöhler discover benzoic chloride, a compound of the same radicle, which by ordinary double decomposition yields the bromide, iodide, and cyanide of benzoyl. Benzoic chloride, in contact with water, becomes converted into benzoic acid, chlorhydric acid being formed at the same time. By acting on benzoic chloride with alcohol, benzoic ether is produced, and by the treatment of the same chloride with ammonia, benzamide, the chlorine in both cases being likewise eliminated in the form of chlorhydric acid.

Our present expressions for oil of bitter almonds' benzoic acid, benzoic chloride, benzoic ether, and benzamide—

Oil of bitter almonds	C_7H_5O,H
Benzoic acid............	C_7H_5O,OH
Benzoic chloride	C_7H_5O,Cl
Benzoic ether	C_7H_5O,OC_2H_5
Benzamide	C_7H_5O,NH_2

are simple translations of the above discoveries into the symbolical language now prevalent. So far as the facts are concerned, nothing has been added, nothing taken away; by our new notation the philosophical interpretation remains unaltered. Benzoyl, in our present conceptions, plays exactly the same part which Liebig and Wöhler assign to it, when in concluding their paper they say :—*

"In reviewing once more the facts elicited by our inquiry, we find them arranged around a common centre, a group of atoms preserving intact its nature, amid the most varied associations with other elements. This stability, this analogy, pervading all the phenomena, has induced us to consider this group as a sort of compound element, and to designate it by the special name of Benzoyl."

Oil of bitter almonds, benzoic chloride, and benzamide, continue to this day to be considered, in conformity with Liebig and Wöhler's view, as combinations of this so-called compound-element with hydrogen, with chlorine, and with a fragment of ammonia respectively ; and if our mode of viewing the composition of benzoic acid and of benzoic ether has been slightly altered, it is because modern chemistry assumes the weight of the molecule of water to be the double of what it was formerly conceived to be ; we are thus led to look upon alcohol as a derivative of water, and to regard benzoic acid and benzoic ether as originating by the simple substitution of benzoyl for an atom of hydrogen in the aqueous and alcoholic molecules respectively.

It may be briefly mentioned here that, in connection with this inquiry, Liebig and Wöhler also

* *Untersuchungen über das Radical der Benzoësäure.* Ann. Chem. Pharm., iii, 279.

examined benzoin, recognising it as identical in composition with oil of bitter almonds, and associating the two compounds as striking examples of isomerism, the conception of which had already been propounded by Berzelius.

Nothing can be more simple and modest than the observations with which the associated investigators introduce their inquiry, marking, as it does, a new era in the history of chemistry :—

"When a chemist is fortunate enough to encounter, in the darksome field of organic nature, a bright point affording him guidance to the true path, by following which he may hope to explore the unknown region, he has good reason to congratulate himself, even though he may be conscious of being still far from the desired goal."*

And well might the two investigators congratulate themselves! To how few is it permitted to open up such a road through the previously thick and trackless jungle! And how seldom does this road lead to a mine so rich in knowledge as the one they have unlocked!

The first of aldehydes, in whose behaviour the characteristic features of the whole species find their type! The first of acid-chlorides, now among our most powerful agents of research, to which—to quote but two among hundreds of illustrations—Gerhardt's anhydrides and Brodie's organic peroxides

* *Untersuchungen über das Radical der Benzoësäure.* Ann. Chem. Pharm., iii, 249.

owe their origin! It is true the preparation of the chlorides from the aldehydes has no longer its former value, Cahours having pointed out a more convenient mode of producing them, by the action of phosphoric chloride upon the acids; but the grand reactions by which these chlorides furnish us acids, ethers, and amides, remain conspicuous conquests, not shaken by time, but still as fresh and fruitful, as on the first day of their discovery.

To return once more to the typical chloride with which Liebig and Wöhler's researches have endowed our science:

We pour benzoic chloride into water which we have rendered alkaline by soda. The solution now contains sodic benzoate, and on saturating the liquid with chlorhydric acid, splendid crystals of benzoic acid—benzoic hydrate—are deposited.

We perform a similar experiment with alcohol; a powerful reaction ensues, and on adding water to the solution, there is separated from the mixture a heavy aromatic oil consisting of benzoic ether—benzoic ethylate.

One more experiment. The large flask before us contains ammonia gas. On pouring benzoic chloride into the gas, the vessel becomes filled with a thick white vapour rapidly condensing into a solid crystalline mixture. Water removes from this mixture the ammonic chloride, leaving behind crystals of benzamide—benzoic amide.

Liebig and Wöhler do not exhaust the new lode they have opened; the vein is too rich in treasure for any two miners to carry it away, and many a glittering ore which tempted them for a moment is left behind. Indeed it is but a part of their glory, that their mine, after having yielded to themselves so much, still left so much to their successors.

Of the great truths bequeathed to subsequent exploration, some attracted, so to speak, a brief glance from Liebig and Wöhler's penetrating eyes. They note, for example, that when oil of bitter almonds is dissolved in alcoholic potash, potassic benzoate is formed. On addition of water, the salt dissolves, and an oily body separates, which is no longer oil of bitter almonds. Remarkable as is this fact, and seductive the line of inquiry which it opens, Liebig and Wöhler push its investigation no further, simply remarking that this oil may originate from oil of bitter almonds by the decomposition of water, the oxygen of which gives rise to the formation of the benzoic acid. Years later (1853) Cannizzaro shows that this oily body is benzoic alcohol.

So again, to take another example, Liebig and Wöhler treat benzoic chloride with pentachloride of phosphorus, and obtain a new liquid organic compound which they examine no further. More than twenty years later (1858) Schischkoff and Rosing prove this liquid to be the chloroform of the benzoyl series.

To take one more instance of the same kind: on heating benzamide with caustic baryta, Liebig and

Wöhler observe, in addition to ammonia, a transparent oily fluid, lighter than water, possessing an aromatic odour and the sweet taste of sugar. What new treasure is it that they have come upon? To us it is no mystery that they have produced the first of that important group of bodies we now call nitriles; but they carry their experiments no further, and it is not till many years later (1844) that Fehling resumes the dropped thread of their inquiry by determining the composition of benzonitrile, which he encounters in a different, but somewhat analogous reaction.

If, even now, after the lapse of forty years, the results of this research exert such a fascination upon us, what must have been the triumphant emotions with which it was greeted by contemporaries? Even Berzelius, whose sober judgment cannot be accused of being easily betrayed into impulsive enthusiasm, declares exultingly that in this research he discerns the dawning splendour of a new morning.

"The facts put forward by you," he writes* to Liebig and Wöhler, "give rise to such considerations, that they may well be deemed the beginning of a new day in vegetal chemistry. For this reason I would suggest that this first discovered radicle, composed of more than two elements, should be named *proin* (from πρωΐ, the beginning of day, in the sense

* *Schreiben von Berzelius an Wöhler und Liebig über Benzoyl und Benzoësäure. Stockholm, d. 2 Sept.*, 1832. Ann. Chem. Pharm., iii, 282.

of *ἀπὸ πρωῒ ἕως ἑσπέρας*, Acts xxviii, v. 23), or *orthrin* (from *ὄρθρος*, day-break), terms whence names like *proic acid*, *orthric acid*, *proic*, and *orthric chloride* could be easily derived."

The various roads thus opened up by the single investigation we have surveyed, would have furnished to most chemists material for a dozen papers, and probably, for half-a-dozen years' work :—not so with the associated investigators. In fact they return only once more (in 1837) to the same field of inquiry by studying the ESSENTIAL OIL OF BITTER ALMONDS.

At that period we find the origin of this oil still unexplained. The researches of Robiquet and Boutron-Charlard have shown that it does not exist ready formed in bitter almonds,—that the essential oil, as well as the prussic acid, appear only after their treatment with water. The same chemists have further proved the existence of amygdalin as a component of bitter almonds. Finally, Peligot has observed that distillation of amygdalin with nitric acid produces oil of bitter almonds. Such are the *disjecta membra* which Liebig and Wöhler have to weld into one whole.

They first make out the composition of amygdalin, which is corroborated by its conversion, when treated with an alkali, into ammonia and amygdalic acid. They next observe that an infusion of sweet almonds, which contains no amygdalin, when added to a solution of amygdalin, gives rise to the formation of oil of bitter almonds and prussic acid, and this fact supplies them with the key to the as yet unexplained process. There exists in bitter as well as in sweet almonds, a substance soluble in water, a kind of vegetal albumin, which on account of its peculiar properties they call emulsin. This, Liebig and Wöhler discover, acts as a ferment, and thereby accomplishes the metamorphosis of amygdalin. But a comparison of the formula of the latter with those of prussic acid and oil of bitter almonds, proves to Liebig and Wöhler that a portion of the constituents of the former remains unaccounted for, and this complementary product Liebig and Wöhler make out to be sugar. They are thus enabled to point out in amygdalin the first of glucosides, a family of substances soon to bring forth a rich harvest of beautiful discoveries. In fact the investigation of amygdalin has been the forerunner of such researches as those of Piria regarding salicin, of Stas and of Hlasiwetz regarding phlorizin, and of those, within our own days, regarding coniferin, by Harmann and Tiemann. As an incidental offshoot of this line of investigation, we may note, in passing, the suggestion by Liebig and Wöhler, that for medical purposes, a weighed quantity of amygdalin, dissolved in an emulsion of sweet almonds, should be employed, instead of the bitter almond and laurel-water up to that time in use for like therapeutic purposes. The proposed medicament furnishing, as it does, a certain and constant, instead of a variable preparation, has, nevertheless, to this day, escaped the attention of medical practitioners and pharmacists, whose notice we take this opportunity of again calling to the subject.

Passing from these joint labours of Liebig and Wöhler on the benzoic group, we have next to consider some equally valuable observations proceeding from Liebig alone.

In the year 1836, Winkler, by the treatment of oil of bitter almonds with chlorhydric acid, obtained an organic acid which on theoretical grounds was subsequently designated FORMOBENZOIC, and is now known under the name of PHENYLGLYCOLLIC acid. Liebig, engaging in the study of this substance, determines its composition, and elucidates the reaction by means of which it is formed. This reaction is a typical one. By it Strecker subsequently succeeded in converting the aldehyde *par excellence* into lactic acid; and since then it has been employed with success in a multitude of researches.

Here also the discovery of HIPPURIC ACID claims our attention. In 1830, Liebig repeats the experiments of Rouelle, as well as the later ones of Fourcroy and Vauquelin, which had indicated the presence of benzoic acid in the fluid secretion of horses. These experiments lead him to the discovery of an acid containing nitrogen, to which he gives the name hippuric acid. He remarks that it may be considered as a compound of benzoic acid with an organic body as yet unknown. The nature of this body it was reserved for Dessaignes to establish, by showing that hippuric acid, when treated with acids, splits up into benzoic acid and glycocoll. I need not remind you of the high theoretical importance of hippuric acid, considered as a typical compound, the model in structure of a host of analogous bodies. Nor will you fail to note with interest the further researches in this direction, which induced Liebig, supposing hippuric acid to be a derivative of benzoic acid, to search for this last named body in the food of horses, the failure of which search led him strenuously to deny the existence of benzoic acid in hay. Of equal interest is Liebig's further statement here, based on a comparison of crystalline forms, that the substance discovered in *Anthoxantum odoratum* by Vogel, is not, as the latter had asserted, benzoic acid. Liebig's view on this point was confirmed many years later by an investigation of this substance by Bleibtreu, who not only proved it *not* to be benzoic acid, but demonstrated it to be identical in composition with the coumarin of the Tonka bean.

Here we may notice yet another beautiful discovery which Liebig made, so to speak, *en passant*. Not long after the celebrated research on oil of bitter almonds, Laurent, investigating the action of chlorine on benzoin, obtains beautiful crystals having the composition of benzoyl, and, indeed, he does not doubt that he has isolated the radicle of the series.

Assuming this interpretation to be correct, Laurent's substance ought to be convertible by potash into potassic benzoate and oil of bitter almonds. In performing the experiment, Laurent obtains, in fact, an acid which he believes to be benzoic, together with an oily substance, which he supposes to be oil of bitter almonds. A closer examination of the reaction shows Liebig that Laurent has been led astray in interpreting the phenomena by a preconceived theory. His benzoyl has no connection with the radicle of the benzoyl series, and is, therefore, henceforth designated by Liebig by the name of BENZIL. The acid observed by him is not the benzoic, but a new acid which Liebig names BENZILIC ACID, the formation and composition of which he so thoroughly establishes that very little has been subsequently added to the history of the compound. The discovery of benzilic acid is thus seen to have been simply elicited by a review of Laurent's investigations. I should not leave unnoticed that it was in the course of pointing out to Laurent the danger of too freely indulging in theoretical speculations, that Liebig told us with such exquisite humour his early misadventure with bromine, already adverted to.

If the examination of the benzoic compounds impresses us like the sudden outburst of a new day's splendour, we may nevertheless be struck with an equally profound admiration by the more slowly developed, but not less brilliant light which emanated from Liebig and Wöhler's grand investigation of URIC ACID AND ITS DERIVATIVES (1838).

These researches present a picture of the most ardent and indefatigable struggle after truths which are but slowly and partially revealed to the investigators. The results of this inquiry are, with the exception of a few points of secondary importance, accepted as exact and truthful even at the present moment; whilst the problems which it left unsolved have been only yet in part worked out by the researches of our later time. To Liebig and Wöhler we owe the path which has conducted us nearly to the goal,—the leading thought which has inspired others to further labours in the same field, and which promises us at no distant date the final dispersion of the clouds still overshadowing this portion of our domain.

Uric acid was not an entirely unknown body when Liebig, in 1834, established its formula. Discovered by Scheele, as early as 1776, in calculi, and subsequently in the urine of man, its presence in the similar excretion of birds, and in guano, had been proved by Fourcroy and Vauquelin. But it was William Prout who, in 1815, when a youth only in his 19th year, discovered the most copious source of this interesting compound, showing that the solid excrement of snakes consists, to the extent of no less than $\frac{9}{10}$ths of its weight, of uric acid. At that period, however, this last-mentioned source of uric acid appears to have been rather inaccessible, as the larger species of serpents were then but rarely exhibited in Europe; so that, even in 1823, Vauquelin considered it worth while to give in the *Annales de Chimie et Physique* a minute description of serpent's solid deposit as a substance rarely seen. Nevertheless, some products of the decomposition of uric acid had even then already been observed. Henry had prepared a pyro-uric acid which, in 1829, was identified by Wöhler with cyanuric acid. Brugnatelli and Prout, about 1818, had discovered the so-called purpuric acid, a mixture of murexide with colourless substances; and Brugnatelli had further pointed out the existence of a peculiar soluble compound called by him erythric acid. But the composition and true chemical character of these compounds still remained shrouded in obscurity.

This obscurity, it was reserved for Liebig and Wöhler to dispel. Engaged together in investigations for this purpose, they soon made it manifest that uric acid possesses an interest by no means exclusively physiological; but that, mainly by reason of the unlimited mutability which is its most characteristic quality, it claims, with certainly an equal force, the attention of the chemical philosopher. This liability to chemical change is, indeed, the very fact which, while increasing the difficulty of the inquiry, enabled Liebig and Wöhler to reap a harvest of results, such as few chemists ever gathered from a single field of research. Not less than sixteen new and most remarkable bodies were at a single stroke, that is to say, in the course of this single investigation, introduced by Liebig and Wöhler into the edifice of chemistry! And it is certainly noteworthy that only one of these numerous substances has, up to the present date, disappeared again from science, and that of their formulæ one only has called for modification. It would be difficult to adduce a proof more striking than this of the skill in manipulation, and scrupulous accuracy with which they conducted their experiments. The inquiry has been subsequently resumed by some of the most distinguished chemists of our time; and the well-applied exercise of their care and skill has certainly done much to elucidate the subject further. But their collective labours have not resulted in the production of a series of compounds nearly so numerous and interesting as that first made known to us by Liebig and Wöhler's united work in this great field.

This circumstance lends a particular interest to the consideration of the method they adopted,—shown by its abundant outcome to be so advantageous. To take, then, a cursory glance at this method of theirs, we may note in general terms, that it consists essentially in alternate oxidation and reduction. They employ as their oxidising agent nitric acid, more or less dilute, and raised to various degrees of temperature. Under these varied conditions uric acid furnishes different products, some of which the inquirers recognise as old acquaintances, such as, for instance, oxalic acid, urea, and allantoïn, the last-mentioned compound, as its name reminds us, being the characteristic constituent of the allantoïc liquid of the cow. In others they recognise substances which their predecessors had observed, but failed to elucidate, and of which the composition, centesimal and molecular, still remained to be made out. In the course of their investigations they show that Brugnatelli's so-called erythric acid is an indifferent body which is formed by the molecule of uric acid fixing one molecule of water together with one atom of oxygen (supplied by cold nitric acid) and losing one molecule of urea. This body Liebig and Wöhler designate alloxan, and they give proof that it takes part in the formation of most of the uric derivatives. With sulphuretted hydrogen they demonstrate that it yields alloxantin, remarkable for its violet reaction with baryta-water, and that this, by further reduction, is converted into dialuric acid. By treatment with sulphurous acid they show, moreover, that alloxan is changed into thionuric acid, which retains the sulphuric acid formed in the reaction; and which, by eliminating this sulphuric acid therefrom, they transform into uramile. Alkalies, they proceed to prove, produce alloxanic acid, as also that compound, marked by the simplicity of its composition, which is called mesoxalic acid. And by treatment with heated nitric acid, alloxan, they further find, yields parabanic and oxaluric acids; but I will not continue the lengthy enumeration of substances evolved in their energetic pursuit of this inquiry by Liebig and Wöhler. Flying time forbids the protraction of the list, interesting as every member of it unquestionably is to the chemist, and deeply important by the varied associations which they together recall to the memory.

Let me, however, before taking leave of this group of bodies, direct your attention yet for a moment to the most beautiful, and at the same time most enigmatical, of all the compounds which owe their origin to uric acid. I allude to murexide, that precursor of rosaniline, forming, like rosaniline, crystals so remarkable for their green metallic lustre,—a body whose wonderful tinctorial properties seemed destined to, and for some time actually did, restore the magnificent purple of the ancients. The existence of murexide had been pointed out by Prout and Brugnatelli; but our information regarding the compound was most scanty and fragmentary when Liebig and Wöhler began its investigation; and it is to these philosophers that we are indebted for the method of obtaining the substance in a perfectly pure condition, as well as for its first analysis, and for the first exact and complete description of its properties.

The necessity I am under of compressing into the narrow compass of a few sentences the history of this remarkable group of bodies, altogether precludes me from more than glancing at the masterly interpretation of their experiments given by the associated inquirers. Suffice it to say, that Liebig and Wöhler looked upon the members of the uric series as combinations of urea with various groups of elements derived from a hypothetical body called urile, which they assumed to exist, united with urea, in uric acid itself. Other theories of the constitution of the uric group have since been proposed; but it deserves notice that the view most generally received at present differs from that of Liebig and Wöhler only as the theory of substitution differs from the older theory of radicles. Liebig and Wöhler considered these substances as compounds of urea with various radicles, whilst we look upon them as urea, for part of whose hydrogen, atomic groups of varying composition have been substituted.

In thus rapidly reviewing the derivatives of uric acid, may I be allowed to quote from Liebig and Wöhler's paper upon this subject a passage clearly indicating how distinctly they foresaw the synthetical direction in which organic chemistry was then about to advance.

"From these researches," they say,* "the philosophy of chemistry must draw the conclusion that the synthesis of all organic compounds which are not organised, must be looked upon, not merely as probable, but as certain of ultimate achievement. Sugar, salicin, morphine will be artificially prepared. As yet we are ignorant of the road by which this end is to be reached, since the proximate constituents required for building up these substances are not yet

* *Untersuchungen über die Natur der Harnsäure.* Ann. Chem. Pharm., xxvi, 242.

known to us; but these the progress of science cannot fail to reveal."

In the sketches of Liebig's experimental labours hitherto submitted to you, I have, mindful of the mixed audience I am addressing, avoided as far as possible the use of formulæ. Were I strictly to adhere to this plan, I should be obliged to leave unnoticed one of his most important inquiries,—one which has materially assisted in shaping the course of chemical thought, and in preparing our present theoretical conceptions. I speak of his researches on the CONSTITUTION OF ORGANIC ACIDS; which, at the same time, elucidated the composition and ultimately fixed the formulæ of some of the substances most frequently occurring in the household of nature.

If Liebig and Wöhler, in examining oil of bitter almonds and its derivatives, achieved in so short a time such a series of triumphs, their success was in a great measure due to the accuracy of the analytical methods with which science had been just endowed by Liebig. A more extended application of these methods promised a further harvest of results. In this respect the extensive group of organic acids appeared to offer an appropriate field for cultivation. Hence malic, maleic, camphoric, quinic, and meconic acids were successively examined, not invariably with absolute success, since the numbers obtained did not always agree among themselves, or with the results of subsequent inquirers. Perhaps, in some cases, it was the rapidity with which one investigation followed another that caused these early failures; but in most cases they may be traced to the faculty some of the above acids possess of uniting with varying quantities of water,—to their tendency, as we now should call it, of forming anhydrides.

The relation between the quantity of water participating in the formation of an acid and its faculty of forming salts—the basicity of an acid—was, at that time, a problem completely unsolved.

Berzelius felt this difficulty when, in 1833, he represented the citrates by the formula

$$C_4H_2O_4,RO,$$

($H = 1$, $C = 6$, $O = 8$), whilst, what was perfectly unintelligible to him, he found citric acid to be, not as he expected,

$$C_4H_2O_4,HO,$$

but

$$C_4H_2O_4,\tfrac{2}{3}HO,$$

corresponding to one-third of the molecule to-day admitted,

$$C_{12}H_8O_{14}, \quad \text{or} \quad C_6H_8O_7$$

in our present notation.

Nor was Liebig able entirely to remove the difficulty, when, somewhat later, referring to the results of Berzelius, he discussed this peculiar discrepancy between the formulæ of citric acid and the citrates.

But in the same year (1833) this obscurity was illumined by a sudden ray of light. Indeed, we find ourselves before Graham's ever-memorable inquiry as to the causes of the varying basicity of phosphoric acid. He traced this variability to the varying quantities of water which can be fixed by phosphoric anhydride. In ordinary phosphoric, or, as we now call it, orthophosphoric acid, this anhydride is united with 3 atoms of water replaceable by bases; in pyrophosphoric acid with 2 atoms; and in metaphosphoric acid with one atom. But how it happened that the same anhydride should in some cases combine with 3 atoms, in others with 2, and in others, lastly, with one atom of water or base, remained unintelligible so long as chemical thought continued to move in the grooves of the dualistic conception.

We now approach the period when Dumas on the one hand, and Laurent on the other, began to sap the foundation of this conception by sketching the first outlines of their substitution theories. The ideas regarding the nature of acids set forth in 1809 by Humphry Davy, and advocated again in 1819 by Dulong, were thus once more brought prominently under the notice of chemists. Liebig opened his mind to these ideas, and the result was the publication, in 1837, of a joint note by him and Dumas on the constitution of several polybasic acids, which is so terse and clear that I cannot do better than quote some of its principal passages.*

* *Note sur la Constitution de quelques Acides.* Par MM. Dumas et Liebig. Compt. rend., v, 862.

"The difficulty which the study of citric acid and its salts presents, can be satisfactorily explained only by assuming that the atomic weight of this acid must be trebled, so that in the neutral salts three atoms of base have actually to be admitted.

"We thus arrive at the following series:—

Absolute citric acid; Anhydride in combination with bases	$C_{12}H_5O_{11}$
Dry acid	$C_{12}H_5O_{11},3HO$.
Crystallised acid	$C_{12}H_5O_{11},3HO + 2aq$.
Barium salt	$C_{12}H_5O_{11},3BaO$.
Sodium salt	$C_{12}H_5O_{11},3NaO$.
Silver salt	$C_{12}H_5O_{11},3AgO$."

The formulæ here quoted are not exactly those which Liebig and Dumas give in their paper: to render them comparable with those previously used for citric acid, I have translated them into the notation originally adopted by Berzelius, differing from that employed at present only in the circumstance of carbon and oxygen figuring in its formulæ with half the atomic weights which are to-day admitted.

"The question of citric acid settled," Liebig and Dumas continue, "we have devoted ourselves with lively interest to another inquiry of the same order. The formula adopted for tartaric acid no longer expresses all the facts which the examination of that acid has brought to light. According to the analysis of Berzelius, tartaric acid contains

$$C_4H_2O_5."$$

"We doubt not the correctness of this formula, but we have good reason to believe that tartaric acid, like citric acid, is capable of losing water at the expense of its constituents."

"Meconic acid presents a similar behaviour. It is obvious that in these cases we have to deal with a new order of phenomena, the study of which appears to lead to the following general rule:—In the formation of citrates, tartrates, meconates and cyanurates, each oxygen-atom contained in the base which combines with the acid, replaces and eliminates in the form of water an atom of oxygen contained in the acid: hence these acids do not form salts with an excess of base, but salts of the same description as those of phosphoric acid.

"Assuming tartaric acid to be a hydracid and doubling its formula, we arrive at the following simplified formulæ, which we place in juxtaposition with the more complex expressions previously employed:—

	Berzelius' formulæ.	Liebig and Dumas' formulæ.
Anhydrous acid	$C_4H_2O_5$	
Hydrated acid	$C_4H_2O_5HO$	$C_8H_2O_{12}$, H_4
Neutral potash salt	$C_4H_2O_5KO$	$C_8H_2O_{12}\left\{\begin{matrix}K_2\\H_2\end{matrix}\right.$
Cream of tartar	$\left\{\begin{matrix}C_4H_2O_5HO\\C_4H_2O_5KO\end{matrix}\right.$	$C_8H_2O_{12}\left\{\begin{matrix}K\\H_3\end{matrix}\right.$
Anhydrous tartar emetic..	$\left\{\begin{matrix}C_4H_2O_5KO\\C_4H_2O_5SbO_3\end{matrix}\right.$	$C_8H_2O_{12}\left\{\begin{matrix}K\\Sb\end{matrix}\right.$

"It would thus appear that anhydrous tartaric acid does not exist, tartaric acid itself being a hydracid of a new description, formed by the union of the radicle $C_8H_2O_{12}$ with 4 atoms of hydrogen. In the saline compounds of tartaric acid, these 4 atoms of hydrogen are entirely or partially replaced by an equivalent of metal."

From a comparison of the formulæ suggested by Liebig and Dumas for tartaric acid and the tartrates with those we use at present :—

Tartaric acid...............	$C_4H_2H_4O_6$
Neutral potash salt..........	$C_4H_2H_2K_2O_6$
Cream of tartar	$C_4H_2H_3KO_6$
Tartar emetic	$C_4H_2SbKO_6$,

it is obvious that the former actually coincide with the latter, if the atomic weights of carbon and oxygen be doubled. Moreover, it is with no small interest that we perceive in these expressions the undoubted germ of our modern notion of the non-equivalence of elementary atoms; for since, in accordance with these expressions, it is the oxygen of the base, which eliminates the hydrogen of the acid, it is clear that the amount of metal replacing a given quantity of hydrogen must vary with the composition of the metallic oxide acting upon the acid; and that if MO represents the composition of an oxide consisting of 1 atom of metal and 1 atom of oxygen, then the metal M^a of an oxide M^aO_2, and the metal M^b of an oxide M^bO_3, must have the value of 2M and 3M, and therefore of 3 atoms of hydrogen respectively. Indeed in anhydrous tartar emetic, it is the univalent potassium-atom which replaces one atom, and the trivalent antimony atom which replaces three atoms of hydrogen in tartaric acid.

The inquiry, commenced by Liebig and Dumas jointly was not continued in this form for a long time. The two investigators found it more convenient to work out the subject separately. Indeed, as early as in the following year (1838), Liebig returned to the question in an elaborate memoir, "On the Constitution of the Organic Acids." In this splendid paper he accurately describes meconic acid and its salts, comenic acid and the comenates, silver citrate and pyrocitric acid (the citraconic acid of the present day), cyanuric acid and the cyanurates, aspartic acid and its silver salt, gallic acid and the gallates, tannic acid, tartaric acid and tartar emetic, racemic, malic and even mucic and pyromucic acids, giving at the same time the full experimental evidence upon which his conclusions as to the nature of these substances are based. The paper more fully developes the views set forth in the joint note with Dumas, which it supports by additional facts and elucidates by new reasonings.

Meconic acid, by the analysis of its silver salt, is recognised as tribasic, and comenic acid, in a similar manner, as bibasic, in accordance with the views at present adopted. Indeed the formulæ advanced by Liebig are, with few exceptions, the same that we admit to-day. It is here that the simple relation, in which, so far as their molecular weights are concerned, the cyanic, fulminic, and cyanuric acids stand to each other, is first pointed out. Particular attention is paid to the diagnosis of polybasic acids. As a characteristic criterion, the faculty possessed by an acid of forming double salts—salts containing two metals—is adduced. This is a valuable indication, but one, as we now know, by no means absolutely to be relied upon, and, indeed, from the non-existence of a potassio-sodic sulphate, Liebig is led to deny the bibasicity of sulphuric acid, since so unmistakeably attested by overwhelming evidence. On the other hand, the appropriate selection of the saline compounds, from the study of which the basicity of an acid may be safely inferred, is of course a subject on which he dwells with predilection. The silver salts are found to be the most trustworthy guides in fixing the limits of basicity. Many acids, which with potassium form only acid salts, are readily converted into neutral silver salts; and this behaviour is at the same time brought forward as a most powerful argument in favour of acids being compounds of radicles with hydrogen; for, only on this assumption, can we understand why silver oxide, which is easily reduced by hydrogen, is fixed by acids in larger proportion than the more difficultly reducible oxide of potassium.

That the theory of hydracids advanced by Davy and Dulòng involves the necessity of admitting a host of radicles which are not isolated, is not, in Liebig's eyes, an argument against it; since the supporters of dualism were not less compelled to assume the existence of numerous imaginary bodies, viz., the acid anhydrides, but few of which had been discovered at that time. The theory of the hydracids, on the other hand, presented the advantage of collecting all acids and salts under the same point of view, and of satisfactorily explaining why equivalent quantities of sulphuric and chlorhydric acid, when acting upon lime, give rise to the elimination of the same quantity of water. An additional confirmation of this theory is furnished, according to Liebig, by the behaviour of silver sulphocyanate. The dualistic conceptions formulate this compound

$$CyS,AgS,$$

whilst according to Davy's and Dulong's view it must be written

$$CyS_2Ag.$$

The latter conception alone explains why sulphuretted hydrogen separates silver sulphide from the compound.

The dualistic views were defended with great pertinacity by Berzelius, who was inclined to explain the varying basicity of phosphoric acid by assuming an isomerism of the anhydride, and Liebig was thus induced to return once more to the subject, maintaining his views in a letter addressed to Berzelius.* In the latter he says, "We have no proof that the water we expel from an acid by the action of a metallic oxide, is as such contained in it; all we know regarding the process is that an equivalent quantity of metal is substituted for the hydrogen."

* *Réponse à M. Berzelius.* Compt. rend., vi, 747.

The chemists among my audience cannot but feel strongly the full importance, for the advancement of our beloved science, of this extensive series of researches on the constitution of organic acids, which, in the scantiest outlines, I have laid before them. Teeming, as they do, with a multiplicity of new facts, elucidating the nature of a large number of substances of widely diffused occurrence, and therefore of paramount interest, they nevertheless claim our attention on still higher grounds. I have already alluded to the fact of their foreshadowing the doctrine of the non-equivalence of elementary atoms, now governing our notions in such a sweeping manner. But there is yet another direction in which these researches have exercised a powerful influence on modern ideas. If we compare the well-defined precision of our present conceptions of Molecule, Atom, and Equivalent with the vague and confused notions prevailing half a century ago, the salutary change must be looked upon as one of the most important features of the progress lately achieved. To this beneficial change the researches of Graham, Liebig, and Dumas on the polybasic acids paved the way; and it is more especially to Liebig that we are indebted for the first unequivocal separation of the notions of equivalent and molecule; for what he terms the atomic weight of an acid, in contradistinction to its equivalent, differs only in name from what we now call its molecule. This separation he advocated at all times, both in his writings and in his lectures, but never more strenuously than when discussing the constitution of the polybasic acids, than which no class of compounds affords more striking illustrations of the difference between equivalent and molecule.

The celebrated paper "On the Constitution of the Organic Acids," is by no means the only contribution which Liebig has made to our knowledge of this class of substances. Were I allowed to indulge in details, it would not be difficult to quote scores of acids, to the history of which he has supplied most valuable information. Let me only remind you that, in addition to the acids mentioned in the previous paragraphs, he studied formic, acetic, lactic, succinic, picric, and camphoric acids; he discovered fulminuric acid—a fourth acid isomeric with the cyanic, fulminic, and cyanuric acids,—which is formed by the action of chloride of potassium on fulminating mercury,—and cyanurenic acid, a compound existing under certain conditions in the urine of the dog. In connection with his researches on alcohol and ether, he examined the salts of sulphovinic and phosphovinic acids; and on the occasion of the memorable inquiry into the nature of fatty bodies, instituted under his auspices in the Giessen laboratory by Bromeis, H. Meyer, Playfair, Redtenbacher, and Varrentrapp, he published a series of most valuable papers on the stearic, margaric, and oleic acids. Nor must we leave unnoticed his joint investigation with Wöhler of that singular organic acid, supplied in the form of its ammonium compound by the mineral kingdom. Liebig and Wöhler's experiments established the simplest atomic formula of mellitic acid; and it is but one more proof of the scrupulous accuracy of the associated analysts, that this expression has been unequivocally confirmed by the splendid series of researches recently published by Baeyer, to whom it was reserved to assign to this remarkable compound, by converting it into benzol, its true position in the system of organic substances.

I cannot conclude this rapid sketch of Liebig's more important experimental inquiries without alluding to the conspicuous results he obtained in the investigation of ALCOHOL AND ITS DERIVATIVES.

I mention these researches last, not that I consider them of less interest than those which have preceded, but simply because, extending over a period of more than twenty years, they embrace some of his later as well as of his earlier discoveries.

Liebig's first experiments on alcohol were made as early as 1832, when he examined its behaviour under the influence of chlorine. Everybody knows that this inquiry, undertaken from purely scientific motives, led to the discovery of two compounds now in continual use for the diminution of human suffering. Let us always thankfully remember that we are indebted to Liebig for the discovery of CHLOROFORM, whose anæsthetic properties, now so admirably applied for the alleviation of pain in disease, enable even the severest operations of the surgeon's knife to be performed on patients lapped in complete unconsciousness, entirely exempt from the torture previously inseparable from such treatment. Nor must we ever forget that it was Liebig also who first presented us with CHLORAL, the benign properties of which for inducing sleep, when other soporifics fail, have rapidly established it in the highest rank amongst the therapeutic agents placed by chemistry at the disposal of medical art. What an illustration of the practical advantages ever flowing from the pursuit of science, even when apparently most abstract!

Liebig, although he failed, at first, in establishing what are now considered the true formulæ for chloral and chloroform, for which we are indebted to Dumas, described the method of preparing these substances, their properties and changes, with an accuracy which it would be difficult to surpass. It is to him that we owe hydrate of chloral, the beautiful crystalline compound now preferred for medicinal purposes. The formation of this hydrate presents a splendid phenomenon of crystallisation, which I am tempted to exhibit to you. The flask before us contains a known quantity of the anhydrous liquid chloral, to which we now add an equivalent proportion of water. The two liquids are well mixed by agitation, when, as you observe, crystallisation immediately sets in, the walls of the flask becoming coated with a beautiful network of brilliant needles of the hydrate. It was Liebig who first observed the remarkable transformation of chloral into chloroform and formic acid by caustic alkali, which also we can easily illustrate by an experiment. For this purpose a small retort containing soda is provided with a condenser and dropping tube. On allowing a stream of chloral to flow in through the latter, you observe a powerful reaction, and on gently heating the retort, chloroform is volatilised; which collects as a heavy transparent oil at the bottom of the water in the receiver. The presence of formic acid in the residue is easily demonstrated by adding thereto a solution of corrosive sublimate and acidifying with chlorhydric acid, when a copious white precipitate of calomel takes place. It was this easy transition of chloral into chloroform, which first suggested to Oscar Liebreich the happy idea of trying the physiological action of chloral; as he expected that the small amount of alkali contained in the blood would be sufficient to produce this change, thus generating chloroform within the organism. Be this as it may, experiment has proved the physiological effect of chloral to be essentially different from that of chloroform, it being, in fact, rather hypnotic than anæsthetic, and possessing therefore a value of its own. It is certainly remarkable that two compounds, which for years presented an interest exclusively scientific, were found at last to be endowed with properties so eminently practical. In 1847, fully 15 years after its discovery, chloroform was used for the first time as an anæsthetic by James Simpson, and 20 years more had to elapse before the physiological action of chloral was discovered.

Liebig's investigation of this substance, searching as it was, had not, probably on account of difficulties of production, been followed up, as it deserved, by chemists, whose laboratories throughout the world would hardly, in 1868, have supplied, collectively, so much as half a kilogramme of chloral. At the present date, only some seven years later, a factory in Berlin alone produces about 100 kilogrammes daily. In an assembly of chemists I need not dwell on the services rendered to science by chloral, since its abundant industrial production has made it available for day-by-day use in our laboratory researches.

The object of Liebig's investigation of alcohol was to elucidate the constitution of this important compound and its derivatives. When he entered the field, Dumas and Boullay's celebrated memoir had already appeared. It is well known that the French chemists had been led to consider ether and alcohol as hydrates of olefiant gas; Liebig, on the other hand, denied the existence of olefiant gas in these compounds, and regarded them as derivatives of a radicle, consisting of carbon and hydrogen, to which he gave the name of Ethyl. The long-protracted contest between the advocates of these rival theories forms one of the most interesting episodes in the early history of organic chemistry. It ended in a signal victory for Liebig, and a universal adoption of his theory. The olefiant gas, or, as it was also called, the etherine, theory,—notwithstanding the new support which Berthelot's memorable transformation of olefiant gas into alcohol at one time appeared to lend to it—has well nigh fallen into oblivion; whilst the theory which assumes ethyl to be a constituent of alcohol and ether is, at the present moment, as much in men's minds as it was when Liebig first suggested it, although our present view of the relation between alcohol and ether has changed from that entertained by Liebig.

Many of those here present will remember that, about a quarter of a century ago, Gerhardt and Laurent advanced their new system of chemical notation; and while some of us witnessed, others perhaps shared, with somewhat of their youthful impetuosity, the contentions excited by the newly-proposed system. Let me remind you that the new ideas of the two French chemists were nowhere sooner countenanced than in this land of free debate; and that, among the chemical warriors who took the field in their support, scarcely one was earlier afoot, and in his championship of the new system, no one more strenuous and successful, than the scientific chieftain under whose colours we are assembled this evening. Indeed, it is chiefly to Odling's efforts, courageously begun in early youth, and vigorously prosecuted in strong manhood, that England owes the honour of having been foremost of all nations to recognise the intrinsic truth and value of the new chemical doctrine. The modification, necessarily introduced by the new notation into the formulæ employed by Liebig to express the composition of some of the substances which he examined, extends also to ether. When Gerhardt and Laurent's notation is employed, Liebig's ether-formula must be doubled; and this change (at the first glance at all events) appears but difficultly reconcilable with the views originally advanced by Liebig. Nor was it until Williamson, by experiments irresistibly convincing, elucidated the transformation of alcohol into ether, that this modification was generally accepted. Thanks to his masterly researches, we now, without a shade of doubt, look upon alcohol and ether as water in which the radicle ethyl has been substituted for one and for two atoms of hydrogen, or—to use the more modern form of expression—as ethyl united with the water-fragment hydroxyl, and ethyl linked to ethyl by the intervention of oxygen. I will not remind the members of a Society to which these classical researches were first presented that, far from invalidating Liebig's ethyl-theory, they have brought that hypothesis to its last and finest development; but I will recall to memory the circumstance that to Liebig, Williamson owed the very agents he so successfully employed in the solution of this problem, viz., the ethylates of potassium and sodium. Strangely enough, Liebig never published a special paper on this subject, but only incidentally, in the course of his controversy with Dumas, mentions the formation, and describes the composition and properties, of these important compounds. When we think of the immense services these substances have rendered in the development of organic chemistry, it is but right that we should gratefully remember the chemist who supplied us with such powerful agents.

I must not linger too long over Liebig's researches on alcohol and ether, but I cannot forego the pleasure of alluding to his deep-rooted conviction of the truth of his conceptions, as displayed by him in the course of some remarks on his attempt to decompose ether with potassium. He says,* "I have no doubt that one day we shall succeed in isolating the radicle of ether, the hydrocarbon C_2H_5. Some experiments I have made upon the action of potassium on ether have failed to yield any decisive results. The metal becomes almost immediately coated with a film of oxide which prevents all further action; but the investigation of the behaviour of potassium with the chloride or iodide of the radicle will soon show how far these conceptions are founded on fact."

Were they founded on fact? Which of us does not know that Liebig's dream, though many years later, was realised by Frankland, who, using the very reactions which floated in Liebig's mind, succeeded in isolating the hydro-carbon radicle of alcohol and ether?

And here again, I hardly need remind the chemists in this assembly that the ethyl of Liebig's conception, and the ethyl we received from Frank-

* *Ueber die Constitution des Aethers und seiner Verbindungen.* Ann. Chem. Pharm., ix, 15.

land's hands, notwithstanding their identity of composition, are two essentially different things. But I will remind them that nearly half a century has elapsed since Liebig endowed science with this fruitful conception. Compare the chemistry of that already remote period with the chemistry of the present day. On the foundations then scarcely visible above the ground, a palatial edifice has been raised, with pinnacles and dome, and busy labourers have added vast wings, not preconceived by the original architects! Among the movements which the growth of chemistry evolved, there is, perhaps, not one more momentous in its consequences than the development, to which I have already alluded, of molecular in antithesis to atomistic conceptions; and on no previous occasion, perhaps, did this antithesis more powerfully take hold of the mind of chemists than when Brodie, in a paper as remarkable for its brevity as for its sagacity, explained to the Chemical Society the difference between ethyl free and ethyl combined, between the molecule ethyl, $C_2H_5.C_2H_5$, and the fragment of that molecule, the group of atoms, C_2H_5, which, associated with other atomic groups, we assume in alcohol and its prolific progeny. And thus we may look upon it as a last, and by no means least remarkable outgrowth of Liebig's fertile mind, that his ethyl-theory, far from having, by the fact of isolated ethyl proving different from what he had anticipated, become irreconcilable with the evolution of modern ideas, has, on the contrary, supplied to chemists the most striking examples for their elucidation.

One more illustration, and we pass away from alcohol and ether. I remind you of an experiment which is familiar to every chemist in this room. The transparent liquid in this bottle, which rapidly sinks to the bottom of the test-glass filled with water into which we pour it, is oxalic ether. On addition of ammonia this ether solidifies to a beautiful crystalline mass, which you all know to be OXAMIDE. The fact of a crystalline body being produced by the action of ammonia on oxalic ether had long been known. Oxamide had also been obtained from ammonium oxalate; but nobody suspected any connection between these two reactions, until Liebig studied the process and taught us the simplest and most elegant method of preparing amides, a method now successfully adopted in hundreds of other cases.

It is impossible to speak of Liebig's labours on alcohol and on ammonia without alluding to the wonderful sagacity which enabled him by a ratiocination, such as true genius only can evolve, to predict one of Wurtz's most beautiful discoveries.

"It is of some interest," says Liebig, in an article on Organic Bases,* "to become acquainted with a view conceived for the purpose of explaining the properties of the nitrogenous organic bases. There is satisfactory proof that the oxygen of these bases has no share in their alkaline properties, every-

* *Handwörterbuch der Chemie*, i, 697.

thing appears to indicate that these properties are dependent on their amount of nitrogen.

"This view is based upon the chemical behaviour of ammonia, which may be regarded as the type of all organic bases, being the one which has the simplest composition.

"The behaviour of ammonia with potassium, with mercuric chloride, and with certain organic acids, incontestably demonstrates that a portion of its hydrogen is replaceable by elements or compound bodies, playing the part of elements. Indeed, we know that potassium and sodium, when heated in ammonia gas, disengage therefrom 1 eq. of hydrogen, for which is substituted 1 eq. of potassium or sodium. These compounds, if amidogen be designated by the expression $NH_2 = Ad$, assume the following formulæ:

Hydramide (Ammonia).	Potassamide.	Sodamide.
H + Ad	K + Ad	Na + Ad

. . . .

"Now we know that amidogen is capable of replacing equivalent for equivalent the oxygen of many organic acids, and we find that the new compounds thus produced have altogether lost the nature of acids, being indifferent in their chemical character."

. . . .

"If the radicles of the oxalic and succinic acids which, when united with oxygen, give rise to acids, do, when in combination with amidogen, form bodies absolutely indifferent, the conclusion appears legitimate that amidogen by its union with compound radicles, which in their chemical activities stand nearer to itself, must generate substances, which, possessing the characters of ammonia, are organic bases."

. . . .

"If in the oxides of methyl and ethyl, the oxides of two basic radicles, we were able to substitute 1 eq. of amidogen for oxygen, there cannot be the slightest doubt that we should obtain compounds perfectly similar in their behaviour to ammonia. Expressed in a formula, a compound $C_2H_5NH_2 =$ EAd, must have basic properties."

Everybody knows that more than ten years later, the substances thus forecast by Liebig's penetrating intellect, were actually produced. Methylamine and ethylamine were discovered by Wurtz, and found to possess all the properties which Liebig's fertile imagination had assigned to them.

I am almost afraid that this long enumeration of facts discovered or established by Liebig may have wearied you; I will therefore, with your permission, from the mass of valuable and interesting subjects which must remain unmentioned, select only, and but for a moment's notice, Liebig's researches on the formation of acetic acid.

That alcoholic liquids, when exposed to atmospheric air, are, under certain conditions, converted into vinegar, had long been a familiar fact, but the exact nature of this process of oxidation remained without explanation. Nor had the question been advanced by the researches of Doebereiner, whose varied experiments served rather to obscure than to elucidate the subject. No sooner, however, had Liebig's perspicacious sagacity devoted itself to the inquiry, than the clouds, so long overhanging the process, were instantaneously dispelled. Liebig showed that the oxidation takes place in two successive phases; the first consisting in the removal of hydrogen in the form of water, by which reaction aldehyde is produced; the second, in the direct addition of oxygen to the aldehyde, which is thereby converted into acetic acid: and in his classical paper on the oxidation of alcohol, he has given an account of this typical process of transformation, so lucid and exhaustive, that but scanty gleanings have remained for his successors. The important intermediate compound which, even to the present day, we call the aldehyde *par excellence*, was introduced to us by Liebig, who, at the same time, discovered another compound closely allied to aldehyde, and scarcely less interesting, viz., acetal. It is certainly worthy of remark that the same hand which gave us the first of aromatic aldehydes, also presented us with its prototype in the fatty series. In the whole range of organic chemistry, it would be difficult to name a compound more interesting and important than aldehyde. Endowed with an extraordinary power of combination,—everybody knows that aldehyde, when poured from one vessel into another, is converted into acetic acid—exceedingly liable to intramolecular changes, when the opportunity of uniting with foreign matter is denied it—capable lastly of processes of condensation which enable the chemist to pass, as it were, by bounds, from one series into another—the aldehyde *par excellence* has, by this rare combination of properties, become an inexhaustible source of discovery, from which even the chemists of the present day frequently and largely draw. This is not the place even to indicate the numerous researches of which aldehyde, at various times, and more especially of late, has been the subject; but I will at all events allude to the share of the harvest which has fallen to Liebig's lot. Metaldehyde,—of which there is a specimen upon the lecture-table, such as few chemists probably have seen,—was discovered by Liebig, who thus supplied an early and most striking illustration of polymerism. He also observed the still enigmatical power which aldehyde possesses of causing cyanogen to fix the elements of water, and to become rapidly converted into oxamide. Aldehyde was a favourite subject of Liebig's research, to which he returned on various occasions, either alone or in association with others. With Wöhler, he examined the action of cyanic acid upon aldehyde, which gives rise to the formation of that remarkable body, trigenic acid, the investigation of which deserves to be taken up again. Another splendid reward of their experiments was the discovery of thialdine, a typical base containing nitrogen and sulphur, which is deposited in large well-formed crystals when sulphuretted hydrogen is passed into aldehyde-ammonia. Also carbothialdine, the crystalline product of the action of bisulphide of carbon upon aldehyde-ammonia, which Liebig discovered and studied in conjunction with his friend and pupil Redtenbacher, deserves a passing notice. Nor should I, in conclusion, forget to mention that it was while studying aldehyde, that Liebig first observed the mirror-like deposition of silver from its solutions. The flask before me contains a slightly ammoniacal solution of silver. Let us have a last experiment. On pouring into this solution a few drops of aldehyde, and gently warming the liquid, the vessel, as you observe, instantaneously becomes coated with a lustrous film of silver, reflecting objects far more perfectly than a mercurial mirror. The process is marked with the simplicity which characterises so much of Liebig's work. It forms the starting point of the manufacture of silver mirrors already spoken of in an earlier part of this lecture; to the further development of which industry Liebig largely contributed by his subsequent researches on the subject.

I fear, Ladies and Gentlemen, that your attention must be well nigh exhausted by the overwhelming mass of matter I have had to compress into the narrow compass of this lecture; and yet I feel how very imperfectly I have done justice to my subject, and how very meagre and fragmentary has been the outline of Liebig's voluminous life-work which I have been able to present to you. That an endless variety of miscellaneous observations; that the long list of bodies the composition of which he determined whilst elaborating his method of organic analysis; that the plant-ash analyses which, during his chemico-agricultural researches, were made either by himself or under his immediate guidance; that the numerous elegant processes he gave for preparing substances; that his technical and domestic preparations —his plan for making unfermented bread, for instance; that the various methods with which he enriched mineral analysis; that the several analytical processes which he supplied to physiologists and medical men; that his analyses of nearly all the more important mineral waters of Germany: in one word, that his minor contributions to chemistry could not have found a place in this sketch, is self-evident. But any one who has made himself acquainted with the glorious career of Liebig must be aware that whole branches of his far-reaching activity have been altogether left unnoticed. I have scarcely alluded to his searching and frequently resumed inquiry into the nature of the organic alkaloïds, a field of research, on which he occasionally broke a lance with Regnault, then working on the same subject; but I may remind you that many of the formulæ of the alkaloïds now-a-days adopted, are based on Liebig's determinations. Nor has that remarkable series of chemico-physical experiments been mentioned, which suggested themselves to Liebig, whilst he was engaged with his researches in animal chemistry. The results of these experiments relating to diffusion, to endosmosis, and exosmosis, and to like phenomena playing an important part in the motion of the juices of the animal organism, are published in a separate pamphlet.* Again, I have not even noticed a field of inquiry which Liebig cultivated with never-ceasing predilection, since so many of his researches on collateral subjects very naturally converged thereto. I am speaking of his study of those grand processes of transformation, by which matter circulating in the animal and vegetal kingdoms, is continually returning to the world's mineral stores. On more than one occasion he developed the peculiar views he had formed of fermentation, putrefaction and decay, a subject to which was devoted even the last paper he ever wrote, an elaborate memoir on fermentation, and on the source of muscular power. We cannot indeed flatter ourselves with having yet arrived at the final solution of these great questions; and it will suffice here to remind you that Liebig was a staunch opponent of those by whom

* In Germany the pamphlet appeared under the title: *Ueber einige Ursachen der Säftebewegung im thierischen Organismus. Braunschweig.* The English edition bears the title: Researches on the Motion of the Juices in the Animal Body. By Justus Liebig, M.D. Edited from the manuscript of the author by William Gregory, M.D. London, 1848.

the lower forms of vegetal and animal life are considered to be the cause of these processes, and whom he facetiously compared to the man who imagined the Rhine to be driven by the row of watermills which he saw across the river near Mayence. And to mention yet one more field of Liebig's life-long labours, which, did time permit, it would be most interesting to survey, let me remind you of his long-continued activity as an experimental critic. The brief sketch, which, in a previous part of this lecture, I have endeavoured to give you of his contest with Gerhardt and Laurent, may have served to impress you with his controversial style; but it does not convey to you the remotest idea of the influence which he exercised by reviewing the researches of others,—by submitting them, regardless of anything except the interest of truth, to the crucial test of experiment, sometimes confirming, sometimes refuting them, but at all times throwing new and unexpected light upon the subjects under discussion.

In the preceding sketch, devoted more especially to Liebig's experimental labours, I have naturally omitted to allude to his purely literary achievements. Of an essentially different kind, this work is not less comprehensive, and scarcely less influential. Every chemist knows the celebrated periodical, "The Annalen," founded by Liebig in early life (1832), and which in later years, for a very considerable period, he published in conjunction with his friends

Friedrich Wöhler and Hermann Kopp. Of this invaluable collection, no less than 165 volumes had appeared at the time of Liebig's death; and there is no journal which more faithfully and more thoroughly represents the progress of chemical discovery during the last half century. For generations of chemists it has been an object of ambition to become contributors to its volumes: they contain all the researches performed subsequently to 1832, either by Liebig himself, or by the pupils of the Giessen school. The present editors have, therefore, both gratefully and wisely decided to retain the auspicious appellation, "LIEBIG'S ANNALEN" on the title-page of the journal.

Another grand literary work, undertaken jointly with his friends Poggendorff and Wöhler, was the publication of the "Dictionary of Pure and Applied Chemistry," * the first parts of which appeared in 1836, and which, after many interruptions, was completed in 1856. For years this work has been one of the principal sources of chemical information. The new dictionary of chemistry which, under the auspices of Prof. von Fehling, has just been started by the celebrated German publishers, Friedrich Vieweg and Son in Brunswick, is founded on the work of Liebig.

* *Handwörterbuch der reinen und angewandten Chemie, in Verbindung mit mehreren Gelehrten herausgegeben von Dr. J. Liebig, Dr. J. C. Poggendorff, und Dr. Fr. Wöhler. Braunschweig, bei Vieweg und Sohn.*

Following closely upon the first volume of the Dictionary appeared the "Handbook of Organic Chemistry." The origin of this book was a peculiar one. Philipp Lorenz Geiger, the author of a once celebrated work on Pharmacy, and one of Liebig's early scientific friends, had died in 1836. In the interest of the widow, Liebig generously undertook to revise the chemical part of Geiger's Pharmacy, when a new addition was wanted. But the strides which chemistry had made in the comparatively short time since the last edition had appeared, were such that Liebig very soon gave up the idea of simply improving the work of his late friend. He began to re-write the book, and indeed the volume on Organic Chemistry is entirely an original work.* Now that more than thirty years have elapsed since its publication—thirty years of wondrously active progress in organic chemistry—we cannot but admit that what had previously been a mass of incoherent knowledge, assumes in Liebig's work, for the first time, the form of a finely articulated science. For thousands, not only in Germany, but in all other countries, has it been the leading thread of Ariadne. Immediately after its appearance, a French translation of it was published by Gerhardt; into English it was translated by Gregory, and published as the

* *Handbuch der Chemie mit Rücksicht auf Pharmacie, von Dr. Justus Liebig (als neue Bearbeitung des ersten Bandes von Geiger's Handbuch der Pharmacie). Heidelberg, bei Winter.* 1843.

part on organic chemistry of the later editions of Turner's celebrated work.

The death of Berzelius, in 1848, involved Liebig in another literary undertaking of considerable magnitude. One of the means by which the illustrious Swede had exerted his powerful influence had been by his Annual Report on the progress of Chemical Science. For many years, translated into German by Wöhler, these annual reports had become, for all investigators, a kind of central source of information, the publication of which was eagerly looked forward to. When Berzelius died, public opinion among chemists, with rare unanimity, designated Liebig as the one to continue the work. It was not without hesitation that he accepted the task; the difficulties of which, from the ever-increasing expansion of the field of inquiry, he clearly foresaw were to augment with every year. Nor did he consent single-handed to take the field. It was not until he had secured the co-operation of his friend Hermann Kopp, the author of the classical History of Chemistry, then Professor of Chemical Physics in the University of Giessen, that the new Report was started. And since the work was by no means to be devoted exclusively to chemistry proper in its several ramifications, but was to embrace also the progress of the collateral sciences, physics, mineralogy, geology, and technology, the two editors induced several other teachers in the University of Giessen—H. Buff, the physicist, E. Dieffenbach, the geologist, C. Ettling, the chemist and mineralogist, F. Knapp, the technologist, H. Will, the chemist, and F. Zamminer, the physicist—to join their labours, and thus, *viribus unitis*, was inaugurated in 1849 that magnificent series of Reports,* which, although the editors have changed more than once,† has continued for upwards of a quarter of a century to supply a record of chemical discovery, such as no other language can boast of. With the observations of a legion of investigators, scattered through a hundred journals, in five or six languages, half the time of the inquirer would, but for a work like this series of Reports, be lost in searching all literature for the information extant on the subject of his investigation. Indeed, every experimenter must feel the debt of gratitude which, for help received even before the commencement of his labours, he thus owes to Liebig.

Of Liebig's great works on agricultural and physiological chemistry embodying, as they do, his most important experimental inquiries, I have already had to speak; but I must not conclude this enumeration of his literary achievements, without alluding to one of his noblest productions, from which, more than

* The German title is: *Jahresbericht über die Fortschritte der reinen, pharmaceutischen und technischen Chemie, Physik, Mineralogie und Geologie. Giessen, J. Ricker'sche Buchhandlung.*

† The reports were published from 1847 to 1856 by Liebig and H. Kopp; from 1857 to 1862, by H. Kopp and H. Will; from 1863 to 1866, by H. Will alone; during 1867 and 1868, by A. Strecker; and since that time by A. Naumann.

once in the course of this lecture, I have had an opportunity of quoting. I mean his "Familiar Letters on Chemistry."* They are a charming illustration of a truly popular work on Chemistry. The book is translated into all modern languages. The English edition, Liebig dedicated to the late Sir James Clark, for whom he, like so many others, entertained the highest esteem and the warmest friendship. Few works of a similar character have had a circulation, and exercised an influence, like these familiar letters, from which accurate chemical notions and a sound appreciation of natural phenomena have penetrated into all classes of society. To the student of Liebig's works, these letters present a double interest, impressing him, as they do, with the seductive elegance of his language, the lucidity of his composition, and the cogent power of his reasoning, and affording him, at the same time, an opportunity of surveying at a glance, as it were, the whole field over which Liebig's active mind has ranged. In these letters, which for the most part first appeared in the well-known South German newspaper, the "Augsburger Allgemeine Zeitung," Liebig used to give from time to time the results of such of his experimental enquiries or

* The German title of the book is *Chemische Briefe*. In England it appeared under the title: Familiar Letters on Chemistry, in its relations to Physiology, Dietetics, Agriculture, Commerce, and Political Economy, by Justus von Liebig. London: Taylor and Walton. The letters were first admirably translated by Dr. John Gardner. Later editions are by Prof. W. Gregory; and the last one (fourth) is by Prof. John Blyth.

philosophic meditations as could be rendered accessible to the general reader; and thus we find him treating in them all the various subjects which in succession engrossed his attention; they contain essays on the philosophy of chemistry, on experimental science, on the results of his chemical investigations in agriculture and animal physiology, on industrial chemistry, &c., in a word, on all topics of philosophical interest, which casual circumstances, such as the perusal of an interesting book, an animated conversation, a stirring event of life, might happen to suggest. Thus the admirable letter* on the spontaneous combustion of the animal body,—exploding for ever the notions floating in the heads of medical men and lawyers but a comparatively short time ago,—was occasioned by a *cause célèbre*, the murder of the Countess of Görlitz in Darmstadt, by her servant man, at whose trial Liebig appeared as a scientific witness for the prosecution, and whose conviction was essentially promoted by the irresistible scientific evidence that he adduced. The letters were first published in a collected form in 1844; since which time the work has gone through many enlarged and revised editions.

To complete the list of Liebig's publications, it is necessary to refer to a large number of controversial pamphlets, chiefly on agricultural subjects;

* Letter XXII of the third edition. In Germany the letter is separately reprinted under the title, *Zur Beurtheilung der Selbstverbrennung des menschlichen Korpers. Heidelberg*, 1850.

to a variety of popular lectures; and to a long series of essays and of academical discourses which, in his capacity of President, he addressed to the Bavarian Academy. Amongst the polemical papers, his long controversy with J. B. Lawes and J. H. Gilbert is best known in this country, on account of the eminent position of his opponents among English agricultural chemists. Liebig has defended his views on the question at issue in a special pamphlet.* Among his essays, two very important papers, "On the state of chemistry in Austria" (1838), and "On the study of the natural sciences, and on the state of chemistry in Prussia" (1840), must be singled out; since they have exercised a most powerful influence on the development of chemical education in those two countries —the Governments of which they impressed with the necessity of providing ample funds for the foundation and endowment of institutions for instruction in experimental science. Lastly, among his academical speeches, as having particular interest for English readers, his discourse "Francis Bacon of Verulam, and the History of the Natural Sciences" may be specially quoted. These essays, lectures, and discourses, together with various papers on subjects of general interest, were collected by Liebig's son-in-

* *Die Grundsätze der Agriculturchemie mit Rücksicht auf die in England angestellten Untersuchungen. Braunschweig*, 1855. The English edition bears the title: Principles of Agricultural Chemistry, with special reference to the late Researches made in England. By Justus von Liebig. London, 1856.

law, Professor M. Carriere, of Munich, who in 1874 published them in a separate volume.*

I cannot presume to suppose that in these cursory sketches of Liebig's scientific, and more especially of his experimental labours, I have conveyed to the chemical part of my audience any fact not known to them before. We were none of us less familiar with our combustion tube than with our balance, with our five-bulb apparatus than with our thermometer. The substances of Liebig's discovery, that we have passed in review, are among those which are of most frequent use in the laboratory. The course which the chemical student follows in performing his mineral analysis is Liebig's. The reactions he taught us are those most commonly employed in our researches. His works on agricultural and animal chemistry are in everybody's hands. His "Familiar Letters on Chemistry," who has not read? You will charge me, I fear, with teaching grown-up men the first letters of their alphabet. I do not deny the imputation. The fault, if it be one, so far from weakening, does much to strengthen my case. We could not more eloquently bear witness to the influence Justus von Liebig has exercised upon the progress of our cherished science, than by frankly acknowledging that his teachings have become familiar to us as "household words."

* *Reden und Abhandlungen, von Justus Liebig. Leipzig u. Heidelberg*, 1874.

Thus far, Ladies and Gentlemen, I have endeavoured to sketch to you Liebig the philosopher, the chemist; can I part from you without alluding to Liebig the man? Had I the power of delineating to you his character as it lives in my grateful memory, you would agree with me that our respect and admiration are by no means exclusively due to him for his scientific achievements. Among the many noble lessons that his great life teaches, we may learn that a generous heart, perpetually solicitous for the good of mankind, is as necessary to the true philosopher (in the highest sense of that comprehensive term) as a penetrating intellect; and that our anxiety to discover abstract laws should never be dissociated, in our hearts and minds, from an anxiety, as searching and intense, to find for these grand laws special applications conducive to the well-being of our race.

The leading feature of Liebig's character is his incorruptible love of truth, repudiating untruth, even in jest; indeed, he well deserves the praise which Cornelius Nepos bestowed upon Epaminondas—*adeo veritatis diligens ut ne joco quidem mentiretur.* This veracity is strikingly manifested by the readiness with which he gives up the opinions he at one time believed to be correct, but subsequently recognises to be erroneous. And it is without reluctance that he thus abandons ideas once cherished and even vigorously advocated; for to persist in views no longer tenable, simply because he once conceived them, appears to him a melancholy proof of incapacity for progress. He admits with perfect candour any errors into which he may have fallen; "there is no harm in a man's committing mistakes," he used to say, "but great harm, indeed, in his committing none, for he is sure not to have worked." And characteristic is the eagerness, I had almost said anxiety, with which he endeavours to correct mistakes once recognised. "An error you have become cognisant of," he once said to me, "do not keep in your house from night till morning."

It would be strange if a man of such disposition should have borne ill-will to those who pointed out or corrected his mistakes. On the contrary, it was a noble endowment of Liebig's generous nature that he welcomed, in the interest of truth, what to most men would have appeared an annoyance. Let me give you a case or two in point.

I mentioned in a former part of this Lecture that the expressions originally adopted by Liebig for chloral and chloroform were erroneous: and that we are indebted to Dumas for the formulæ of these important compounds, which are now generally admitted.

How does their discoverer receive this emendation of his results? Listen to what he says himself on the subject, when, at a later period, in a scientific dispute with another, he wishes to impress his antagonist with his views regarding experimental controversy. "As an excellent illustration of the mode in which errors should be corrected," Liebig* says, "the investigation of chloral by Dumas may fitly be adduced. It carried conviction to myself and, I think, to everybody else, not by the copious number of analytical data opposed to the not less numerous results which I had published, but because these data gave a simpler explanation, both of the formation and of the changes of the substances in question. To analytical data alone, no one—and Dumas least of all—would have attached the slightest importance."

Even more striking, perhaps, because of the much more important question at issue, is the example of ready submission to correction, which he gave in the early stages of his agricultural inquiries. It is well known that his first mineral manure, which was manufactured, in 1845, according to indications he considered as the practical embodiment of his theoretical researches, and which was to present to the farmer "the elements of the ashes of the plants to be grown," proved an utter failure. The cause why he failed has long become obvious. Liebig, fearing that the soluble alkaline constituents of his manure might be washed away by the rain-water percolating through the soil, had endeavoured to render these constituents less soluble, by submitting his fertilizing compound to incipient fusion in a reverberatory furnace. This fear, very legitimate according to the

* *Bemerkungen zu dem Aufsatze über die Constitution der Zuckersäure, von H. Hess.* Ann. Chem. Pharm., xxx, 120.

knowledge of the time, was soon recognised to be without foundation. Only a few years later (from 1850 to 1855), John Thomas Way discovered the absorptive power of soils, which enables them to withdraw the plant-food from its aqueous solution, as it percolates the earth. Liebig at once perceived and acknowledged the immense importance of this discovery, although it required him to modify, in some of its essential features, the theory of plant-nutrition he had originally advocated. He confirmed and amplified, by numerous experiments of his own, Way's observation, which, indeed, acquired its full development only after Liebig had pointed out the grand part which the absorptive power of soils has to play in the economy of nature.

If, notwithstanding this marked readiness to accept correction whencesoever it might come, we find Liebig almost continually engaged in scientific warfare, the source of this warfare is, after all, the same love of truth. Any opinion he considers to be true, he will support and defend with an ardour little short of passion; and woe to adversaries who should avail themselves of disingenuous artifices, or irritate him by unjustifiable subterfuges; he will flame up in sudden zeal, and—who would deny it?—occasionally overshoot his mark; but then, even in the next moment, his better judgment returns, and, bent on reconciliation, he is ready to bring the contest to an amicable conclusion. And, the dispute once settled, all angry feeling, which the excitement of the strife might for a moment have created, seems utterly sunk into oblivion. Indeed I shall never forget the glowing delight with which Gerhardt described to me the friendly reception he met with at Liebig's hands, when, some time after the fierce collision to which I have referred, he visited him at Munich. And here, perhaps, I may also fitly recall the noble sentiments which Liebig uttered when, immediately after our late war with France, at a time when the waves of irritation were still running high, he addressed the Bavarian Academy. Although, of all German chemists, the one who had been most frequently in dispute with our French colleagues, he was, nevertheless, the first to hold out the hand of reconciliation, the readiest to soothe the hostile feelings of the moment by an appeal to the glorious traditions of the past.

"This is, perhaps, a fit opportunity," says Liebig,* "of declaring, on the part of our Academy, that a hatred of race (*Stammeshass*) between the German and the Latin nations does not exist.

"We look on the heavy affliction which in former times the French nation has caused to Germany as on an illness, the pains of which are utterly forgotten with the return of health.

"The peculiar nature of the German, his knowledge of languages, his appreciation of other nationalities, compel him to do justice to foreigners, so much

* At the meeting of March 28, 1871. *Reden und Abhandlungen*, p. 333.

so as occasionally to become unjust to himself; and thus we cannot possibly underrate the debt of gratitude we owe to the great philosophers, mathematicians, and natural inquirers of France, who, in so many departments, have been our masters and exemplars.

"When, 48 years ago, I went to Paris for the purpose of studying chemistry, I was fortunate enough to gain, by an accidental circumstance, the attention of Alexander von Humboldt, whose recommendation induced Gay-Lussac, one of the greatest chemists and physicists of his time, to honour me, the youth of twenty, by the proposal of continuing and completing with his co-operation an inquiry I had already commenced. He received me into his private laboratory as a pupil and fellow-worker, an event which has shaped the course of my life.

"Never, indeed, shall I forget the kindness which the German student met with at the hands of Arago, Dulong, and Thenard; and how many of my German countrymen, medical men, physicists, and orientalists could I name, who, like myself, remember with gratitude the active support in the attainment of their scientific aims, which was liberally accorded to them by the French savants.

"Warm sympathy for all that is noble and great, and disinterested hospitality, are among the finest features of the French character. It will be on the neutral ground of science that the best minds of the two nations must meet in endeavouring to reach the high goal common to both, that these sentiments will be kindled again into life and activity; and thus the feeling of fraternity in science, which can never be entirely extinguished, will gradually contribute to mitigate the bitterness with which the deeply-wounded national pride of the French is filled by the consequences of the war they have forced upon us."

In speaking of Liebig's character, it is delightful to remember the absence of anything like personal vanity. Few scientific men, probably, have been equally loaded—I might almost say overwhelmed—with honours. Scarcely an academy or learned society that did not consider it an honourable duty to elect him a member long before he had reached middle life. The highest scientific distinctions which England, France, and Germany can bestow, the Copley medal, the Foreign Associateship of the French Institute, the order *pour le mérite,* were in his possession. Thankfully as he had accepted these honours, they had so little changed the simplicity of his character that many of his intimate friends were ignorant of his having received them. His feelings as to such outward tokens of approbation are well expressed in a letter addressed to his friend, Thomas Graham, on the occasion of his receiving from this country a gift of honour, which he highly appreciated. When, in 1852, Liebig left his professorship in Giessen for the purpose of accepting an academical position in Munich, his friends in England associated, under the auspices of Thomas Graham, in order to present him with a mark of recognition.

The correspondence between Graham and Liebig, to which the presentation of this testimonial gave rise, deserves for more than one reason to be remembered, since it reflects honour on both men. In his capacity as Chairman of the General Committee, Graham accompanied the transmission of the testimonial with the following letter :—

"SIR, *London, July*, 1854.

"Your retirement from the Chair of Chemistry, in the University of Giessen, has appeared to many in this country a fitting occasion for the public acknowledgment of your eminent scientific services. Accordingly a numerous body of your friends and admirers have united to present to you a Testimonial, commemorative of their profound and unalterable regard. In the list of subscribers hereto annexed, you will recognise, with those of your pupils and personal friends, the names of many other gentlemen eminent in science, in social position, and in the practical arts of life, who were anxious to join in this just tribute to your merit.

"In presenting to you this Testimonial, the subscribers desire to express their sense of the benefits which your genius and labours have conferred upon mankind, in adding to the world's stock of positive knowledge. These benefits are limited to no one people or time; but it is felt that Englishmen may, with propriety, take the lead upon this occasion, as the impulse which you have given to Chemical Science has been experienced especially in England. More students from this country than from any other land beyond the bounds of Germany, have worked in the Laboratory of Giessen, and have derived incalculable benefit from the instruction there imparted, and from the noble example there presented to them of an elevated philosophical and scientific life. In England, also, have the applications which you have made of Chemical Science to the cultivation of the soil been peculiarly appreciated and adopted. Your discoveries in practical agriculture have enriched the land, and with you originated the method of scientific inquiry which is here pursued on an extended scale by numerous investigators, and which is rapidly changing the features of the most ancient and important of human arts.

"We earnestly hope that your life, which has been devoted to the highest aims to which man can aspire, may be prolonged to many years of happiness and honour.

"Signed on behalf of the subscribers,

"THOMAS GRAHAM,
"*Chairman of the General Committee.*

"*To Baron Liebig.*"

Liebig replied :—

"SIR, "*Munich, July* 20, 1854.

"The man of Science generally knows of no other reward for the time he has devoted to the discovery of truth and to the investigation of the laws of Nature's powers, than the mental satisfaction which springs from the consciousness of having, to the best of his ability contributed his part towards the advancement of human happiness and human welfare; for toils like his, attended as they are with so many difficulties and sacrifices, and with such mental effort and fatigue, cannot be priced in the market or sold,—cannot be performed to order, or turned into money. If he has been fortunate enough to have gained by his successes the acknowledgment and esteem of his contemporaries, he has obtained the highest object of his ambition.

"If I have laboured for the period of almost a human life, in promoting the progress of Chemistry, and in making its principles subservient and useful to other branches of knowledge, more especially to the industrial Arts and to Agriculture, I gratefully acknowledge that I have received in return all that a man could justly aim at. My satisfaction in this respect is not a little enhanced, when I look back to the number of zealous and able men in whose education I have been enabled to assist, and who are now occupying, in various countries, a distinguished position in the front rank of science, and are, with splendid success, cultivating and extending her domain,—teaching, diffusing, and successfully applying those principles of investigation which constitute the true foundations of scientific progress. It is with pride that I am able to add that, in these, my former pupils, I have gained an equal number of warm friends, who, I am sure, look back with pleasure to the time when we combined our powers in one common aim and effort.

"And now, in addition to all that a benevolent destiny had already granted me in measure above many, I receive from my friends in England, in this gift of friendship, in this testimonial of honour, a token and a proof of their recognition and approbation of my labours.

"When I reflect that whatever of good a man accomplishes, flows from an inner impulse of which he is often but imperfectly conscious, and that a higher power has a part in all his labours of usefulness,—giving to them their life-germ and their capacity of growth, I must own that, in receiving this noble Present, I am blessed far beyond my deserts.

"I feel myself in the highest degree honoured and most deeply touched by this substantial and permanent expression of the kind feelings of my friends in England. Convey to them all my best and warmest thanks. This Gift of Honour possesses for me inestimable value, and will remain a lasting memorial in my family.

"Dr. Justus von Liebig.

"*To Thomas Graham, Esq.*,
"*Chairman of the General Committee.*"

One of the last paragraphs of Liebig's letter is particularly interesting, touching, as it does, on subjects regarding which the views of so profound a thinker can be indifferent to no one. It is only from passages scattered throughout his writings, that it is possible to infer the position held by Liebig towards the grand enigmas, the solution of which, vainly aimed at by generations past, will equally baffle the curiosity of generations to come. But rarely did Liebig unfold the ideas he had formed of God and immortality. With regard to the latter, more especially, he seemed to hold the opinion of Goethe, who thought that the best mode of preparing for the life to come was to do well the business of this.* The concluding passage of his letter to Graham, however, shows distinctly how deeply he was convinced of a supreme power regulating the affairs of this world.

More at length, though perhaps not more explicitly, he has expressed his views in his Familiar Letters on Chemistry :—

"Were a chemist to submit a house to analysis, he would state its composition scientifically to consist of silicium, oxygen, aluminium, calcium and of a certain quantity of iron, lead, copper, carbon, and the elements of water. But this would not convey the most distant idea of the construction of a house. The calcium, carbon, and oxygen of the mortar ; the silicium, aluminium, and oxygen of the bricks ; the carbon, hydrogen, and oxygen of the wood, do not play the part of elements in the structure, but they are present in the form of mortar and stone in the walls, as glass in the windows, as wood in the tables and seats. It is only when combined in the form of wood, stone, glass, &c., that these elements contribute to the construction of the house.

* *Eckermann, Gespräche mit Goethe in den letzten Jahren seines Lebens*, I, 122.

"If any one assured us that the palace of the king, with its entire internal arrangement of statues and pictures, started into existence by an accidental effort of a natural force, which caused the elements to group themselves into the form of a house,—because the mortar of the building is a chemical compound of carbonic acid and lime, which any novice in chemistry can prepare,—because the stones and glass consist of silicium, aluminium, calcium, potassium, and oxygen, united by chemical affinity, and indebted to the force of cohesion for their solidity,—because, therefore, chemical and physical forces play a part in the construction of the house,—we should meet such an assertion with a smile of contempt, for we know how a house is made. Its outer form, its inner arrangement of rooms, &c., proceed from the architect. He constructs the actual house after the plan of an ideal house which exists in his own mind. He realises the ideal creation of his own mind in the building by forces which are produced in the organism of man, and which impress into the service of this ideal creation the chemical and physical forces from which the building material has received its properties. Everywhere the existence of a house presupposes the ideal perception of the house in the mind of some one who is its builder or cause, which sets other forces in action in certain directions, or in a certain form, in order to gain the object in view."*

* Familiar Letters on Chemistry, 4th edit., 285.

In the preceding sketch I have given you some glimpses of Liebig's character, so far as it is reflected in his writings and in his demeanour towards scientific contemporaries. I need scarcely add that the same nobility of thought and generosity of feeling which mark the various stages of his scientific career are also manifested—how could it have been otherwise?—in all the relations of every-day life. Every word, every gesture, was the expression of affability and kindness, although a measured dignity would keep at a distance the profane. In his intercourse with intimate friends he displayed a cordial simplicity irresistibly captivating; towards his pupils—notwithstanding the dignified composure of his deportment—an affectionate kindness, encouraging even the most timid beginner, and assuming towards the assiduous worker the form of a helpful sympathy which shrunk from no sacrifice, and lasted far beyond the period of personal intercourse.

Nor is it only by friends and pupils, or by those whom he was wont to meet in society or in the transactions of ordinary life, that the countless manifestations of genuine interest, of ever-ready counsel, of active support, are thankfully remembered. It was impossible to come even into casual contact with him without being deeply impressed by the generous disposition which prompted him to help where help was wanting, alike whether the seeker was a friend or stranger.

Although Time's creeping hand upon the dial warns me not to venture into detail, I bear in memory a little incident so charmingly illustrative of Liebig's genuine goodness of heart, that I am tempted, if your permission be given, to relate to you this characteristic anecdote.

Many years ago (in 1853), Liebig was making an excursion among the mountains of the Tyrol; and I and two others of his friends had the happiness of being his companions on the tour.

In the course of our rambles one morning we overtook an old soldier who was travelling slowly along the road, much wasted by fatigue and obviously enfeebled by disease. As we came up with him he accosted us with a piteous tale, and humbly implored our aid. Following Liebig's example, whose purse on such occasions was ever as freely open as his heart, we made up among us a little stock of florins, which the poor man evidently regarded as a small fortune dropped by Providence into his hand; then pushing forward, we soon left him behind, and in half-an-hour's time reached a village inn at which we agreed to rest ourselves and dine.

While thus engaged, we observed our poor wayfarer also enter the inn. It pleased us to reflect that, for this once, at all events, he had the means of procuring a comfortable meal; and, having finished our own, we resolved to take a short *siesta* before setting out again on our journey. After some half-an-hour's doze, I awoke and found two of my companions fast asleep in their chairs, whilst Liebig, to my surprise, had disappeared. I immediately got up, and, proceeding to the bar, inquired of the innkeeper where our friend, the elderly spare man of our party, had gone. The landlord replied that the gentleman had been inquiring, a little while ago, for a pharmacy; and that, upon learning that there was none in the village, nor any nearer than in the next village over the hill, he had set out on foot in that direction. Not without some little anxiety at the temporary dispersion of our party, I at once proceeded on the road which Liebig had taken. After half-an-hour's walk, I observed his figure on the brow of the hill, and hurried forward to meet him, impatient to learn the object of his solitary promenade. He answered me simply, that he had perceived in our poor soldier symptoms of low fever, such as quinine was certain to cure, and that he had been over to the nearest pharmacy to get some of this remedy. On his arrival, he added, the apothecary chanced to be absent; but his wife had given him (Liebig) the free run of the bottles, with permission to select therefrom any article he might desire, paying, at his own price, for whatever he might take. He went on to tell me that, fortunately, he had discovered the quinine-bottle, and made up, with a portion of its contents, a boxfull of powders, sufficient, he hoped, for our wanderer's perfect cure. After another half-hour's walk, the powders were delivered to the soldier, with instructions how often they were to be taken. Not a word was said of the long walk they had cost the kindly donor. After receiving the poor man's expressions of gratitude, and promise to obey the instructions given him, we immediately resumed our journey, and I observed, that though Liebig had been toiling over the hills while we slept, he was not, during the remainder of our walk, the least cheerful and buoyant of the party.

This is but one of many touching pictures I could give of this great man's noble simplicity of character and genuine self-sacrificing kindliness. We lads had given the poor sufferer our coin a-piece, and then had gone to sleep, considering our duty done. The master had noted the wayfarer's illness, and resolved on striking at the root of his distress; to which humane end he had generously sacrificed his own much-needed hour of repose.

Is it to be wondered at if we, his former pupils and ever-devoted friends, in admiring the chemist also loved the man?

And here, Ladies and Gentlemen, I may fitly bring my discourse to a conclusion by again pronouncing the great name which, in accordance with the nature and occasion of this lecture, I have had, on its very threshold, to invoke. In this theatre, hallowed as it is by the memory of Faraday's genial presence, how could I speak of Liebig's generous character, of his benevolence, of his kindliness, of his simplicity of life, without reminding you in how high a degree these eminent moral qualities adorned also the character of Faraday? In contemplating the life-long work and conduct of these two great contemporaries, our admiration for both is at first naturally called forth by the marvellous capacities of their commanding intellects, soaring, each in its special sphere, far beyond contemporary effort, and resembling one another in the extent of their penetrating and inventive power. Pursuing the comparison, we are finally struck with the profound resemblance of the two great discoverers in all the highest, purest, and most beneficent qualities which can adorn and dignify the human heart—in their never leaving unassuaged any form of human want or suffering it was within their power to relieve, and never, even in the zenith of their world-wide celebrity, wearing their high honours with undue pride, but being always ready, with child-like kindness and simplicity, to welcome and enlighten the youngest and humblest inquirer in the mysteries of philosophic truth.

Of Faraday's generous disposition in this respect, I am favoured by the kindness of Mr. F. O. Ward, with the means of laying before you a touching example.

My friend, anxious as he expresses himself, to contribute a little mite of his own towards our affectionate commemoration of Faraday's goodness of heart, has handed me an autograph letter dated June 16, 1834, and addressed by Faraday to him, then a lad in his teens, studying science at King's College, London. This lad, like many others of his age, had indulged in a day-dream about the nature of matter, the shape of atoms, and the probable relation of their form to their respective chemical properties. With boyish pride (I am quoting my friend's own recital of the anecdote), the budding philosopher is eager to submit this visionary intellectual ramble to the greatest scientific authority of the age, and probably not less confident than solicitous of obtaining the master's approval for this, his first young venture on the darksome sea of the unknown. Accordingly, with the audacity belonging to his period of life, though wholly a stranger to Faraday, and not even supplied with a letter of introduction to him, he forwards to this Institution, then Faraday's home, the statement of his wanderings in philosophic dream-land, with a request for Faraday's opinion of the theory propounded, and his advice, whether it would be worth the writer's while to test its value by experiment. Nine men of genius out of ten, my friend very truly remarks, in the maturity of their mental powers, and overwhelmed, as Faraday then was by the instant pressure of work in hand, and continued meditation on new experiments and prospective discoveries, would probably have consigned this application to the waste-paper basket. The tenth, perhaps, willing to discharge, at any rate, the claims of courtesy, might possibly have paid off the childish applicant with a few lines of cheap flattery in reply, or advised a round of elementary experiments as ballast for his next sea venture, and so, at small cost, satisfied conscience. How does Faraday treat his boyish correspondent? I hold in my hand the reply which the world-famed philosopher is at the trouble to write to the unknown youth, who, an unintroduced stranger, has addressed him. It is the original paper, and no copy, that I hold up before you; those near me, you observe, recognise his handwriting at a glance, and I will read you the words, as they flowed from Faraday's pen:—

"*Royal Institution*,
"16*th June*, 1834.

"Sir,

"I have no hesitation in advising you to experiment in support of your views, because, whether you confirm or confute them, good must come from your exertions.

"With regard to the views themselves, I can say nothing about them, except that they are useful in exciting the mind to inquiry. A very brief consideration of the progress of experimental philosophy will show you that it is a great disturber of pre-formed theories.

"I have thought long and closely about the theories of attraction and of particles and atoms of matter, and the more I think (in association with experiment) the less distinct does my idea of an atom or particle of matter become.

"I am, Sir,
"Your very obedient servant,
"M. FARADAY."

I have not a word of comment to offer on this noble letter of the master philosopher of the age to the boy whose young mind solicited his aid.

My task, I feel, is now complete; so far, at least, as the narrow limits of my time and of my abilities have enabled me to make it so.

Entrusted by your kind confidence with the duty of bringing to you my country's aid on this occasion, and naturally choosing for my theme the work of Germany's greatest chemist, Liebig, I have but rarely, and, so to speak, incidentally been able to approach the subject of this evening's commemoration—Faraday. Nevertheless, more than one opportunity has presented itself of comparing, at intervals, the grandeur manifested alike in the noble natures, moral and intellectual, of these two ornaments of our day and generation; and I indulge in the hope that, although it has been my task to lay before you the comprehensive life-work of the German chemist, the true object of this evening's meeting, viz., the celebration of the memory of the British physicist, has never been lost sight of. We have contemplated, if I may use this expression, in the portrait of the one the features of the other, and we separate with the unalterable conviction that, in whatever country of the world, in whatever epoch of futurity, mankind shall seek for models of a pure and noble human existence, no two exemplars will in any age stand forth more dignified by their intellectual work, more conspicuous for their moral beauty, than those whose names we have been commemorating this day—MICHAEL FARADAY—JUSTUS LIEBIG.

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST MARTIN'S LANE.

Giessen 19 Dec. 1844

Dear Faraday

I intended to have written you long ago of my safe arrival and that I had found my wife and children well. The opening of my winter course and a mass of work which had accumulated during my absence, have hitherto prevented my fulfilling my intentions. Now however that I have a few days of rest during the Christmas holidays, I will not let the opportunity slip, of wishing you, with my whole heart, a merry Christmas and happy new year. Often do my thoughts wander back to the period which I spent in England; among the many pleasant hours of which, the remembrance of those passed with you and your amiable wife is to me always the dearest & most agreeable. With the purest pleasure I bring to mind my walk with her, in the botanical garden at York, when I was afforded a glance of the richness of her mind: what a rare treasure you possess in her. The breakfast in the little house with Snow Harris and Graham, and our being together at Bishopthorpe are still fresh in my memory. I wish it were only my good fortune to see and talk with you oftener and to exchange ideas with you.

Nature has bestowed on you a wonderfully active mind, which takes a lively share in everything that relates to Science. Many years ago your works imparted to me the highest regard for you which has continually increased as I grew up in years and expanded in judgment and now that I have had the pleasure of making your personal acquaintance and seeing that in your character as a man, you stand as high as you do in Science, a feeling of the greatest affection and esteem has been added to my admiration. You may hence conceive how grateful I am for the proof of friendship which you have given me.

I have every reason to be satisfied with my journey in Great Britain, rare proofs of recognition have indeed been given me. What struck me most in England was the perception that only those works which have a practical tendency awake attention and command respect, while the purely scientific works which possess far greater merit are almost unknown. And yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here in the eyes of scientific men no value, or at least but a trifling one is placed on the practical results. The enrichment of science is alone considered worthy of attention. I do not mean to say that this is better, for both nations the golden medium would certainly be a real good fortune.

The meeting at York which was very interesting to me from the acquaintance of so many celebrated men, did not satisfy me in a scientific point of view. It was properly a feast given to the geologists, the other Sciences serving only to decorate the table. The direction, too, taken by the geologists appeared to me singular, for in most of them, even the greatest, I found only an empirical knowledge of stones and rocks, of some petrefacts and few plants, but no Science. Without a thorough knowledge of Physics & Chemistry, even without mineralogy a man can be a great geologist in England

I saw a great value laid on the presence of petrefactions and plants in fossils, whilst they either do not know or consider at all the chemical elements of the fossils, those very elements which made them what they are.
This letter has already grown too long and truly I fear to weary your patience. I cannot however deny myself the pleasure of expressing a sincere wish to see you and your wife here in Giessen next summer. Did you know how quietly we live at our German Universities, you would certainly expect from your visit only benefit to your health. Except scientific pursuits we have no other excitements of the mind. We take walks in our beautiful green woods and in the evening drink tea at the neighbouring old castles. This is our recreation. I beg of you, dear Faraday,

to listen to my request, I pray your dear wife to assist me in trying to make you decide on this journey. My wife unites with me in begging this, it would give her the greatest pleasure to make the personal acquaintance of you and your Lady.

Farewell, dear Faraday, preserve to me your friendly favor and believe me with all sincerity to be

your very truly
Dr. Just. Liebig

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